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Editorial Notes

AT the end of the year the Editor becomes critical and asks himself—How far is ANTIQUITY fulfilling its purpose? Does it 'serve as a link between specialists and the general public'? Do its articles give non-technical information about important new discoveries? Is our outlook modern and our criticism constructive? We ask ourselves these questions because we know that our readers also may ask them, and because our circulation, upon which our existence depends, requires that the reply should be favourable. Obviously we cannot give an impartial reply ourselves, but we know, from the letters we receive and the steady stream of new subscribers, that ANTIQUITY does satisfy the demands of many who are interested in archaeology. That interest is varied, and we try to give in each issue something that will appeal to each section. But archaeology is world-wide; our subject is the origin of man and the roots of our culture, and important discoveries bearing on these matters may come from any part of the world.



It is quite proper that one's chief interest should be centred in the prehistory and early culture of one's own country, and since ANTIQUITY is published in Britain and has a large circulation in that country, we try to include an article on British archaeology in each number. During 1955 we have published five such articles dealing with the origins of our own culture. But it is possible to combine that restricted interest with a wider one covering the whole sphere of human origins. Of course the central problem is the origin of man himself, and here we may be on the verge of a solution. At the recent Pan-African Congress, described in this number by Sonia Cole, some exciting new discoveries were reported; and in our next number we shall publish a full account of them by Kenneth Oakley.



The same policy is followed in the Notes. Some of our readers may wonder how we reconcile with that policy the publication of rather detailed descriptions of discoveries in Southern Ethiopia? The answer is that that region was, down to quite modern times, on the fringe of civilization; it was the point of contact between the oriental civilization of the Old World, represented by Christianity and Islam, and the still barbaric culture of Africa. The penetration of that civilization, proved by the discovery of inscriptions,

had already been recorded, but the record is buried in obscure publications and its importance has been overlooked. The civilizing influence was short-lived, but for several centuries it was a reality, as the ruined towns of Somaliland prove (see *ANTIQUITY* xi, 315-27). We meet with similar phenomena in Europe in the last days of the Roman Empire; but the contacts there established were less evanescent.

We also lay claim in our last leaflet, to have 'influenced the development of archaeological method' during the nearly thirty years of our existence. That claim is justified not only by the publication of articles on method, but also by the occasional discussion of archaeological organization. That concerns the layman because, in the last resort, it is he who provides the money (and often the labour also) which alone makes archaeological research possible. The great development of State-supported archaeology in recent years is not an unmixed blessing, though it is of course, inevitable and has come to stay. Its danger lies in the creation of a bureaucracy, some of whose members may lack the disinterested enthusiasm of the true archaeologist. There is a marked difference in this respect between different nations. In some there exists a multitude of small societies composed of individuals who are to a greater or lesser extent genuinely interested. Not all of them do original research, but all may be counted upon, in an emergency, to provide a body of articulate public opinion. If used with wisdom and discretion, that opinion is a most valuable ally. Thanks to it, and to the co-operation of archaeological societies and individuals with the State, we here have been able either to avert the destruction of ancient sites or to secure their excavation before destruction, and instances could be quoted of similar successes recently achieved in other countries. Unfortunately there are also examples of failure. Government officials are apt to take a dim view of the claims of antiquity when they do not at the same time subscribe some other purpose, such as tourism. It can, however, often be shown that tourism is well served by protection and excavation; and if sites are to be excavated accommodation must be provided for the finds in properly equipped museums which are also tourist attractions. This may not be a very high ideal, but it is a practical one that can often achieve results; in such matters we must behave as 'children of this world', and get what we can by worldly appeals and methods.

There is a corresponding obligation to make museums as attractive as possible by means of clear legible labels, good lighting and display. There has been an immense improvement in this respect during recent years, and today the chief obstacle is expense. It is a vicious circle, because public interest, as proved by the number of visitors, does not usually become evident until after the reorganization of a museum. Our remarks apply more to other countries than to our own, where the museums are now very much better than formerly. But of course generalizations are impossible; there are good live museums in every country. One of the best we have ever seen is in the island of Lipari, whose Professor Bernabo Brea has converted the former fascist political prison into a museum to exhibit the antiquities he has excavated on the citadel immediately outside the building, and in cemeteries round it. Thanks to its admirable arrangement and labelling one can acquire in a very short time a knowledge of the chief prehistoric cultures of the island which his sound stratigraphical excavations have made a key-site for the whole Western Mediterranean.

EDITORIAL NOTES

It was a stroke of genius to start excavations there at all. There were no previous finds, but merely the obvious suitability of the citadel—a rocky acropolis overlooking the sea and harbour—for primitive occupation. Only a few stray potsherds found by Professor Brea at the foot of the cliff gave any indication of prehistoric occupation. Later it was a Greek fortress, and a Greek tower is still visible, embodied in the Spanish Battlements. In a paved open space between the former prison and the churches, there were revealed round stone huts of several different periods, one built above the other. These have now been put in proper order, and on the wall above them are two boards; on one is a plan in several colours showing the different periods (the oldest going back probably into the 3rd millennium); on the other is a diagrammatic exposition of the different strata (see frontispiece). The visitor can thus see at one glance both the plan of the site and the site itself, and he has only a few steps to go to see, in the museum, the objects found, arranged under strata and periods. In one room are cases in which typical potsherds are set out in layers one above the other, exactly as found and to a scale of 1 : 2.



That is the way to interest the public, and it has the advantage of also making the task of the student far easier. By thus displaying the results of his excavations Professor Brea has of course greatly strengthened his case for yet further support from his authorities. It should be added that, while the prehistoric sequence is the most striking feature of the site, there are also reconstructions of jar-burials in a couple of rooms, and last, but by no means least, a really magnificent collection of painted Greek vases of the best period and by the best artists, obtained from the local Greek cemetery.



At the invitation of the American Anthropological Association and the University Museum, University of Pennsylvania, the 5th Congress of Anthropological and Ethnological Sciences will be held at Philadelphia, from 1 September to 9 September 1956. One paper will in principle be accepted from each participant, without prejudice to additional contributions requested for presentation at General Sessions. Abstracts should be in hand by 1 March 1956. No papers lasting longer than twenty minutes will be accepted for publication, and publication cannot be guaranteed. Rooms in University buildings will be available to members at \$2.50 per night. The registration fee is \$10.00; relatives of members may become Associate Members, the fee being \$3.00. Inquiries should be addressed to the Secretary, American Organizing Committee, International Congress of Anthropology, National Academy of Sciences, National Research Council, 2101 Constitution Avenue, Washington 25, D.C., U.S.A. Cable address: NARECO.



Last June the Editor's autobiography was published under the title 'Said and Done', Weidenfeld & Nicolson, 7 Cork Street, London, W.1, £1 1s). Chapter 14—'The Foundation of ANTIQUITY, 1926-8'—gives the first detailed account of how this journal came into existence.

Stories about Fakes

by the ABBÉ BREUIL*

AT the end of the last century, when I was a student at the seminary of S. Sulpice in Paris, and was studying the Bronze Age in the Somme basin, a Monsieur Boulanger, a notary of Peronne, who was then unknown to me, sent me some books unasked, and also a nice water-colour drawing (by Pilloy) of a palstave found in that region. Then later on I received a curious letter from his chief excavator of the Merovingian graves near S. Quentin. It was addressed to 'M. l'Abbé Breuil, Superior of the S. Sulpice Seminary' (where of course I was merely a pupil) and asking me, as being supposed to have correspondents in Rome who were members of the fraternity of S. Sulpice, to put him in touch with them so that he might buy, at a good price, a fish of Roman glass of the Christian period like one of which he had sent me a photographic print with no provenance given. This seemed to me to be suspect.

About this time I paid a visit to Monsieur Boulanger during my holidays. I found a strangely regal figure seated behind a huge table in his office. With an air of great importance he showed me the palstave, which was the only thing in his house that interested me: and he escorted me to the Museum, which was destroyed by German bombardment in 1914. With it there perished an interesting hoard of Late Bronze Age implements, the most important of which I drew and published. He also showed me a remarkable collection of bronze implements whose type and patina were unfamiliar: amongst them were several long triangular daggers with a cylindrical tang, of which I know of only one really authentic example found in the Department of Oise and in a peasant's collection. There are also some in England. It seems very doubtful whether such a series with a pale green patina really came from Oise.

Several years passed, and at the close of the Exhibition of 1900 Dr Capitan asked me to come with him and look at a collection of bronze implements from Lower Egypt in the keeping of an Egyptian dealer who was packing up unsold material. I found that he had the same daggers with the same patina, some having their hafts of hard black wood perfectly preserved. The Eastern Mediterranean origin of such objects was evident.

Many years later I wrote to Monsieur Boulanger begging him to tell me the exact circumstances in which he acquired this collection. He replied that he had bought them from a dealer and on being pressed he told me his name. I then wrote to this man and he replied that he did not know the origin of the objects, which he had bought at a sale following the death of a former naval officer. I may add that they could have been brought back from Alexandria or some other port in the Eastern Mediterranean. I deposited all the documents relating to this affair in the archives of the S. Germain Museum.

During the exhibition at the Petit Palais in Paris (whose date I did not note) of some private collections of Roman glass, Dr Capitan asked me to go with him to look at an extraordinary collection sent by Monsieur Boulanger, consisting of Roman glass said to have come from his excavations in the Department of Aisne. Dr Capitan told me that none of the objects came from there. Then I remembered that letter from Monsieur

* This translation has been read and approved by the Abbé Breuil.—Ed.

STORIES ABOUT FAKES

Boulanger's chief excavator about the fish and the Christian glass from Rome. Dr Capitan then took me to see a dealer in the little street to the left of S. Germain des Prés and asked the shop-keeper to tell us what she knew about this fine series of objects. 'Monsieur Boulanger', she replied, 'bought that collection of Roman glass from me; it all came from Syria, and I sold it to him as coming from there.' Once more I sent a note about the incident to the archives of the Museum of S. Germain en Laye, to be attached to the affair of the bronze implements. I know that during the 1914-18 war Monsieur Boulanger sold this collection to the Germans. I do not know under what description they were sold nor to which museum they went, but it will be obvious that no confidence can be placed in any provenance assigned to them by Monsieur Boulanger.

THE BENOIST AFFAIR

In 1900 when I began to wander about in Berry I went several times to S. Gaultier (Indre) where I was introduced to a very poor fellow, a dentist, who was a competent geologist and had found in known sites a very fine series of fossils. Amongst them were some of silicified *Spiriferina* whose internal whorls, excellently preserved, he had cleverly exposed by using acid. He had also exploited a site well known locally which yielded fossil tortoises. Local prehistory was one of his hobbies, and he had formed a small but valuable museum in his house. From the Magdalenian rock-shelter of Saint Marcel, situated a few hundred metres from the railway station of Argenton and separated from a deep railway cutting by a small by-road, came a very fine series of Magdalenian objects of ivory, bone and reindeer horn together with the usual flints. I asked to be shown the place where they were found; this was certainly authentic, and a great many bits of bone and flint could still be seen lying about. It did not then occur to me that fraud was possible, and I published a note in *L'Anthropologie*, 1902, pp. 145-65, arranging for the principal objects to be bought for a moderate price by the National Museum of S. Germain, where they are still. Monsieur Champion, a first-class sleuth when it came to the detection of fraud, accepted the objects as genuine without any hesitation. No one suspected them until now except myself, and that only recently. During the last few years Dr Allain has resumed work on the site, or rather in its immediate neighbourhood, and during his excavations he found collapsed galleries yielding an older Magdalenian—I think Magdalenian II and III instead of IV and V found by Benoist. I thought I ought to tell him about my suspicions which were at least worth noting and for which I now give my reasons.

Some of these reasons go back to the time when Benoist was young; he died about 1910, and I think he was then at least 60 years old. When he was a young medical student at Bordeaux, he was already interested in prehistory and geology, and he 'discovered' a so-called lake-dwelling; the finds were published by Dr Delforterie. I examined these in the Bordeaux Museum in 1939; all of them, including the polished axes and worked bones, are rather obvious fakes. But Benoist did better than this; he got some Eocene shells from the famous Grignon site near Paris and planted samples of them in the Miocene quarries in the 'faluns' Sands near Bordeaux where they were recognized and collected by local conchologists. This fraud was exposed (though I do not know how) and Benoist was proved to be the author of it. The affair ended so badly that he was obliged to leave Bordeaux and continue his studies elsewhere—I think in Nancy. It was a bad start! What object could Benoist have in mystifying the Bordeaux geologists? I do not know; in any case it was not financial and looks like deception for the mere fun of deceiving—a sort of mythomania. However, that was a long time ago, in the last

century. Benoist had certainly not made a fortune, and in Berry he passed for a poor inoffensive man without ambition, with difficulty supporting a wife and two daughters. But one significant fact aroused my suspicions once again. He sent me two bronze axes—palstaves if I remember right—stated to have come from the neighbourhood of Le Blanc (Indre) and from other named sites in the same department. They were good and authentic specimens and I recognized them, for only a few months before I had drawn them at Montmorillon (Vienne), a very few kilometres from Indre but found at sites which were quite different from those named by him. I said nothing, but I remembered to have seen in his house a rather good specimen of a Bronze Age III sword which he said he had found in a wood north-east of Argenton in a tumulus revealed by the cutting down of a copse. He told me that the cutting had taken place a very few years before. One day I went to Argenton to see him, giving him no warning, and asked him to show me the tumulus. He made no objection, but took me to a wood of oak scrub far older than the date at which he said his find had been made. It is quite possible that these things were found in the neighbourhood, but they did not come from the tumulus—which he was unable to find—any more than did the markedly brachycephalic skeleton which he associated with this sword. I knew then that he had falsified their provenance. I did not say anything to him, and a few months later he died carrying his secrets with him to the grave. His collection, except for the things from S. Marcel mentioned above, sold to the S. Germain Museum, were bought by the Bourges Museum; there was an ornamented vase which he said contained a collection of small bronze objects, and also some twisted gold wire which I had also published, together with the vase, in the *Revue Archéologique* (II, 1902, 22-38). In my 1928 bibliography I added a note after the title 'Une cachette Hallstattienne à Argenton (Indre)', to the effect that, judging from later information, it was probably a fake. I think the vase came from Alsace, and the rest of the objects I fear he made himself. Dentists have suitable tools for carrying out such delicate work; they also have agile fingers and drugs to oxydize copper, and I am afraid that at S. Marcel too he salted the site with objects from various places or of his own making. He was an excellent craftsman. A fluorine test would have been useful to clear up doubts about the objects of worked bone and ivory. Even if these objects should be found to stand up to such a test, it is still not certain that they all came from there. Nevertheless it is certainly curious that he did not mix up objects having nothing in common derived from different Magdalenian periods, for he was ignorant of such precise typology. Whatever may be the reason, he did not fake in order to make a money profit, but rather from some strange passion for mystification which brought him no gain.

The Indus Civilization : a review*

by MAX MALLOWAN

WHEN we look back over the past century of archaeological discovery, about a dozen ancient cities loom large in the mind's eye, a gigantic sequence of silhouettes which in turn inform us of their distinctive civilizations. Mycenae, Troy, Boghaz Koi, and now Mohenjo Daro and Harappa are signal examples of cities at which the brave work of the early pioneers was followed by the activities of another generation. These later workers, with an evolved and perfected technique, probed anew and established a more solid and elaborate edifice of knowledge on foundations which their gallant predecessors had often left insecure. If scientific work continues to keep pace with public interest we should be about to enter on another phase. Then the consolidated work of the second generation which has left so many tempting points of attack for a third, will be illuminated by new discoveries no less rewarding. Indeed what lies under land and water is still an immense treasure, for the recovery of which we are now better equipped than ever.

The part played by Sir Mortimer Wheeler in initiating and perfecting many of the scientific techniques appropriate to field archaeology is common knowledge. In his work on the Indus Civilization the results of his methods are manifest. Clean as a surgeon's knife, Sir Mortimer's controlled instruments of excavation cut into a mound and delicately dissect the complicated succession of laminations which represent the growth, maturity and decay of once civilized organisms, if one may so describe the heaped up deposits of ancient settlements. The section across the western defences of the citadel at Harappa, executed in 1946 (facing page 19) is a classic example of the clear pictorial analysis by which the order and sequence of ancient installations may be determined. Modern scientific investigation, which requires both patience and constant skilled supervision, has as its inevitable corollary the proposition that digging must be slow and unhurried. In parts of the site where the ancient patient may, as it were, be in a critical condition, only a limited number of operators can be employed. On big city sites, however, discrimination is often required as to where on the one hand the brake must be applied, and where on the other some sacrifice may be made for the sake of obtaining the more substantial evidence upon which public interest must be sustained.¹

Fortunately Sir Mortimer has been endowed with the gift of combining detailed analysis with broad synthesis. The Indus Civilization makes good narrative, and the style has that personal punch which we have come to expect, often reinforced with sharp touches of humour.

Not the least among the attractions of this notable book is the succinctness with which the evidence has been marshalled. It is a remarkable achievement to have covered

* *The Indus Civilization*, by Sir Mortimer Wheeler, C.I.E., pp. 98, Plates 24, Text figs. 13. The Cambridge History of India : (Supplementary volume). Price 18 shillings.

¹ Thus the study of the development of Harappa pottery has still far to go in spite of the large corpus which the original excavations yielded. Some progress in this respect was achieved at Mohenjo Daro in 1950, see page 71, but 'the details remain to be explored and worked out in connection with further deep digging, and the task is well worth the considerable labour which will have to be expended upon it'.

so much ground in so small a space, and to have found the time to investigate and relate the collateral evidence from as far afield as Mesopotamia, the Mediterranean and Egypt. So long as the Indus script remains undeciphered and dated documents fail, the chronology must depend largely on comparisons with Mesopotamia. The head and feet of the civilized Harappan patient have to be fitted into a chronological bed somewhere between 2500 and 1500 B.C., not without some forcible stretching and compressing, but it is clear that the upper end goes back a long way further still. No one is likely to quarrel with the author's broad chronological conclusions on the evidence at present obtainable; but in spite of the significant rarity of lapis lazuli, on which there is a most interesting discussion, it is in my opinion clear that links with India are already evident in what is known as Early Dynastic III within the Tigris-Euphrates valley. Particularly important in this respect are the stone vases, sometimes illustrated with scenes which in Mesopotamia appear to be exotic and Indian, yet in India itself would no doubt be described as not quite true to form.² These things perhaps originated somewhere in Iran or Baluchistan and became slightly distorted when copied on either side of these boundaries.

When we review the assemblage of Indus Valley remains, what strikes a foreign observer most forcibly is the 'Indian' character of that civilization as a whole. Most sharply defined is the attachment to water, lustral bathing in tanks, the ability to construct elaborate drains, and to rein back the violence of the Indus river. Is not tank an Indian word? Then comes the sacred bull; the Siva-like figures; even the trappings of the females represented by the terracottas; the phallic *linga*; the system of weights based on fractions of one-sixteenth, still recognizable in the relation of the anna to the rupee. Considered in combination these things appear to an outsider as an Indian tapestry utterly alien to any that he could find in the great 'cultures' elsewhere. To the 'expert' versed in the later history of India such comparisons appear superficial, loose, inexact; but in that respect I am inclined to think that the inexpert is the better able to see the wood for the trees. Sir Mortimer's appraisal of this problem here seems to be eminently sane. In passing it may be noted that we owe to him the thorough excavation and identification of the remarkable and uniquely Indian building west of the Great Bath at Mohenjo Daro, formerly described as a *hammam* with hypocaust, now recognized as a granary with elaborate loading facilities and ventilation. Such a diagnosis might have been expected from the protagonist in 'Animal, Vegetable, Mineral'.

The list of small antiquities, beads, seals, rare pot-fabrics and metal implements which may be used to illustrate the exchange either of goods or of techniques and ideas between the Near and the Middle East is surprising, no less for its length than for the variety of its contents. Every year something more appears to strengthen the liaison, and we may note one of the most promising additions, too recently made for inclusion in this book—an Indian-type seal discovered on the sea shore at Bahrain in the Persian Gulf.³ This welcome news was mentioned by Mr M. T. G. Bibby last year, when he gave

² See *Syria* xxx, 1953, p. 203, fig. 4 for an illustration of the most recent addition to this set of vases, an early Dynastic fragment, discovered by A. Parrot during his eighth campaign at Mari. The most notable examples of these quasi-Indian stone vases come from the Diyala valley. The author refers to the relevant discoveries in Sumer.

³ The author draws attention on page 81 to an important observation made by Vats, that in Mound F, at Harappa, miniature seals are characteristic of the lower levels. My recollection is that most of the Mesopotamian seals comparable with the Indian are relatively small in comparison with the standard Indus-valley type though larger than the miniature. As many, if not most, of the Mesopotamian seals are relatively early, Sargonid and third Ur dynasty, it may be worth while reinvestigating their dimensions. See also p. 85.

THE INDUS CIVILIZATION

a preliminary account of the Danish Bahrain Expedition (1953-54) to the 23rd Congress of International Orientalists at Cambridge. Another possible connection which has recently come to our notice is a marked similarity of style between some of the painted vases of the post-Harappa cemetery H, and the painted urns of Giyan II which may be dated c. 1550-1400 B.C. or thereabouts; a by no means impossible date for the impoverished settlements which were contained within the ruins of the Harappan cities, when the administration had broken down and there no longer existed the will or the power to maintain civic life on its once imposing scale. That brings us to the close of the story and with it to the bold assumption at which Sir Mortimer has on sound logical grounds arrived, that the new comers were none other than the Aryan tribesmen first historically recorded in the sacred books of the Rig Veda, the founders of a new way of life in India. The problem is to find the 'Aryans' and therefore Indra 'stands accused'. The gist of the argument is that Mohenjo Daro and Harappa fit with the picture of the forts which the invading tribesmen of the Veda were described as overcoming, and indeed that there are no later visible installations to take their place as alternative citadels of attack. If the evidence suffices to accept the theory that the last stages of Harappan civilization may be assigned to the middle of the second millennium B.C., the proposition is a reasonable hypothesis, for we know from West Asiatic inscriptions that the 'Aryan' dynasties of the Hittites and the Mitanni were already strongly entrenched in Anatolia, N. Syria, and N. Mesopotamia respectively at that period. But we need a counsel for the defence before we can make a final judgment, and some not uninteresting difficulties come to mind.

The picture presented by the early hymns of the Rig Veda is of a highly organized tribal society, disciplined, militant, skilled in arts and crafts. If we are to identify the rather poor remains which succeeded the heyday of the Indus valley cities with the handiwork of that society, then the archaeological evidence appears hardly to accord with the literary. This discrepancy between the material and the written remains leads me to wonder if in fact the 'Aryan' tribes had not infiltrated into the Indus valley some time before the great period of its urban life had entered a decline. The history of Western Asia about 2000 B.C. and in the century succeeding it is marked by waves of tribal immigrations which were to have profound political and cultural repercussions. The Amorite and Hittite kingdoms are outstanding consequences of these movements, which led to an inevitable clash between the groups of peoples speaking Semitic languages on the one hand, and those affiliated to the Indo-European on the other.⁴

From this time onwards the 'Aryan' way of life began to exercise an increasing dominance in the civilized communities of the north central Mediterranean, Greece, and Anatolia, and was beginning to penetrate into Syria and even to Mesopotamia, where the 'Umman-manda' were already proving a menace to Babylon. The dynasty of Mitanni with its Indian gods, equestrian nobles and chariotry centred on Washshukkanni was another of the successful 'Aryan' groups which appears to have set itself up in north Syria, perhaps about the same time as the Mycenaeans of L.M. II gained a powerful foothold in Crete.⁵

These 'Aryan' societies were tribal in form yet highly successful in the development of civic life, as we may judge from the splendid structures of Mycenae and Boghaz Koi.

⁴ What constitutes linguistic affiliation is indeed hardly a subject which an archaeological reviewer should venture to discuss, but neither can he altogether avoid it. See the suggestive and enlightening article in *Archiv Orientalni* XVII, 1949, 251 ff. by Vittore Pisano, *La Question de l'Indo-Hittite et le Concept de Parenté Linguistique*.

⁵ See Wace. 'The Discovery of Inscribed Clay Tablets at Mycenae'. *ANTIQUITY*, June, 1953, pp. 84-6.

That is why we may legitimately wonder if an early wave of 'Aryan' had not already entered the Indus cities some centuries before the decline, and had participated in their prosperity rather than their degradation.⁶

The inevitable strife between a pre-Aryan urban society and an Aryan nomadic society would have made the task of maintaining the Indus valley cities against the violent encroachments of the flood-water increasingly difficult, and it may be that the newcomers, like the early Assyrian tribesmen moved easily between city and tent and had less to lose than those whose urban roots were of a much greater antiquity.

Such speculations are indeed playing with fire and involve us in difficulties of definition, linguistic, literary, racial and the like far beyond the scope of this brief review, which has already outstripped its proper limits. One more point, however, we must allow ourselves. If the remains of the Indus valley cities present a picture which is already Indian, does not the theory that the 'Aryans' of the Rig Veda had been established there nearer to 2000 than to 1500 B.C. account much more easily for the transmission of prehistoric ideas, however much modified, down to the historic dynasties of a much later age?⁷

It is obvious that a long and sustained period of archaeological activity will be required before we can arrive at a scientific exposition of the 'Aryan' penetration. The reader will find at the end of the book a list of sixty Harappan sites, many of them still unpublished, the detailed examination of which is bound to lighten a horizon which still remains obscure. Once again we owe a debt to Sir Mortimer who has fearlessly tackled a problem to which others less brave would only have advanced negative conclusions, and has thus led the way to what should be yet another fruitful period of archaeological, historical and linguistic research.

⁶ The so-called Jhukar ware which succeeds and perhaps overlaps the Harappan at Chanhudaro may well have some bearing on this problem, for associated with it were circular 'button seals' or seal-amulets which seem to be strongly Cappadocian, perhaps Hittite in character. The old view mentioned by Wheeler (page 44) that at Chanhudaro the Harappan culture is an intrusion into a local continuum could thus be taken as supporting evidence for the suggestion that an 'Aryan' element had long been present in that part of India.

⁷ The chronology of the Rig Veda is as much in dispute as that of the Homeric poems. Many authorities were prepared to suggest a date of c. 1200-800 B.C. for the older hymns, and some have acknowledged that an even higher date is admissible. Owing to the remarkable, indeed exceptional strength of oral tradition in India it seems however by no means unreasonable to suggest that the Vedic writings may accurately reflect the picture of a society many centuries, perhaps even a millennium, older than the date at which they are alleged to have been composed. Such a proposition would be far more difficult to accept for western epic poetry. The kind of problems involved have been discussed in a remarkable essay by Sir Maurice Bowra in 'The Comparative Study of Homer' (*A.J.A.* 54, No. 3, 1950, 184 ff.) For a different kind of attack see *loc. cit.* p. 162 f. 'Some Oriental Glosses on the Homeric Problem', by W. F. Albright. These two articles will lead any reader to recognize how difficult it can be to reconcile archaeological with literary evidence.

Third Pan-African Congress on Prehistory

Livingstone, July 1955

by SONIA COLE

PROBABLY every one of the hundred or so delegates who attended the Third Pan-African Congress on Prehistory would agree that it would be hard to imagine a more successful meeting. The organization was perfect, the papers read were mostly of an exceptionally high standard, and throughout the congress and excursions an atmosphere of good humour prevailed. We have returned rather bewildered from the number of sites visited and the amount of new knowledge gained, but with the satisfactory feeling that African prehistory has made enormous strides since we last met in Algiers in 1952.

At the opening ceremony in Livingstone on 22 July, the Governor General pointed out that this was the first international congress to be held in the Federation. Office bearers for the period 1955-9 were elected as follows:—

President : Dr L. S. B. Leakey.

Vice-Presidents : Prof. C. van Riet Lowe.

Prof. C. Arambourg.

General Secretary : Dr J. D. Clark.

Section I (Quaternary Geology) President : Prof. G. Mortelmans.

Section II (Human Palaeontology) President : Prof. G. H. R. von Koenigswald

Section III (Prehistoric Archaeology) President : Prof. L. Pericot-Garcia.

Sub-committees for each of the Sections were appointed to deal with particular problems, consisting of the President and two Vice-Presidents of each Section together with two additional members. Before discussing papers presented at the congress, some of the main resolutions adopted at the closing plenary session on 28 July may be summarized.

(1) The East African climatic and stratigraphical sequence (from Kageran to Nakuran) is recognized outside this area only where confirmed by at least two of the following lines of evidence : geological, palaeontological and archaeological.

(2) The position of interpluvials within that sequence is now clarified : the upper limit of each climatic division is defined by the onset of the next pluvial phase.

(3) Four Quaternary faunal stages are recognized in Africa:—

Omo-Kanam—Kageran pluvial.

Lower Olduvai (Beds 1 and 2 Olduvai)—Kamasian pluvial.

Upper Olduvai (Beds 3 and 4 Olduvai)—Kanjera pluvial.

Post-Olduvai—Gamblian to Nakuran.

Again, there must be two lines of evidence (as defined above) in the use of these terms.

(4) Until correlations with Eurasia and America have been established, the use of the terms Lower, Middle and Upper Pleistocene should be avoided in Africa (Early, Later, etc. being substituted).

(5) Workers in Africa have been using the term 'Kalahari sand' indiscriminately for sands of various phases of distribution ; the term 'sands of Kalahari type' is to be used instead.

(6) More elasticity being required in the use of the terms 'Early Stone Age', 'Middle Stone Age' and 'Later Stone Age', two transitional groups have been added: 1st intermediate follows the E.S.A., 2nd intermediate follows the M.S.A. Thus in some areas the Fauresmith and Sangoan would fall into the 1st intermediate, while the Magosian, for instance, would be included in the 2nd intermediate.

Space does not permit of a description of the other resolutions adopted, which were mainly of a more local or personal nature.

The invitation of the Belgian government to hold the next congress in Leopoldville in 1959 was accepted gratefully.

Concerning the papers read at the congress, the most important included symposia on Pebble Cultures, on the Australopithecinae, and on Kalahari sands; also papers on the Florisbad, Hopefield and Ternifine sites.

SYMPOSIUM ON THE PEBBLE CULTURES OF AFRICA. Professor van Riet Lowe spoke on the Kafuan culture, Dom Anciaux de Faveaux and Professor G. Mortelmans on pebble cultures of the Katanga, and the papers were followed by a valuable discussion. The need for extreme caution in the acceptance of pebble tools was emphasized by several speakers; they should be accepted, in fact, only when the work of Nature can be eliminated. There appears to be no doubt at all that a pre-Kamasian pebble culture exists in Africa, though much that has been called 'Kafuan' in the past cannot be admitted. Dr K. P. Oakley stressed the need for caution with respect to particular specimens and sites. Pebble tools in the Vaal gravels are indisputable and some of the Australopithecine deposits are certainly later than them; but chipped pebbles from the Makapan Limeworks Cave, for instance, should not be accepted too readily as artifacts. The nature of the material—dolomite—makes them particularly difficult to interpret as it weathers so easily; the issue should be decided only when enough pebble tools have been recovered made of harder materials such as quartzite. Several speakers supported Oakley's conclusions on the Limeworks material and Dr Leakey emphasized the danger of misinterpretation in gravel deposits; when pebble tools are found in clay on the other hand, the possibility of flakes having been removed by natural agencies can be ruled out. Professor Mortelmans mentioned several possible sources of error in interpretation, one of which is the effect of glacial action (some of us were able to examine Pre-Cambrian tillites during the Katanga excursion and see the appearance of glacially chipped pebbles).

SYMPOSIUM ON THE AUSTRALOPITHECINAE. A satisfactory agreement seems to have been reached between workers in Africa and elsewhere on the dating of the Australopithecines. This was largely due to Dr Oakley's conclusions on the dating of the breccias at Taung during his visit in 1953. At that time it was thought that Taung was the oldest of the Australopithecine deposits. Oakley showed that the tufa at the base of the cave must have been formed under pluvial conditions, and that the aeolian sand contemporary with *Australopithecus* must have been deposited *after* a pluvial phase, i.e. it could not be earlier than the Pleistocene; it could possibly be inter-Kageran; but it more probably dates to the Kageran-Kamasian interpluvial. Since then, several workers in South Africa have confirmed that the Australopithecine breccias cannot possibly be earlier than the Villafranchian (Kageran) and in some cases they are probably later.

This conclusion has been reached by the Abbé Lavocat in his studies of the rodent fauna, by Mrs R. F. Ewer on the carnivora, and by Dr C. K. Brain working on the palaeoclimatological approach. In an outstanding paper, Dr Ewer demonstrated the time sequence of the Australopithecine breccias to be as follows: Sterkfontein, Taung, Makapan, Swartkrans, Kromdraai. Dr Brain's work (described below) has shown that

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Makapan is probably later than Sterkfontein and Dr Ewer has come to the same conclusion. Previously it had been thought to be the earliest of the deposits (Oakley, 1954). Also there appears to be a greater time gap between Swartkrans and Kromdraai than had been supposed. Her conclusions on the relative ages of the deposits are based on the number of species of carnivora, primates, pigs and insectivora common to each. There is as yet insufficient evidence for absolute dating by this method, though judging by the percentages of extinct and living species, the Australopithecine deposits as a whole seem to be not significantly later than Omo (although the difficulty of comparing fauna from cave sites and open sites should not be overlooked).

Dr Brain's studies of the mineralogy of the Australopithecine breccias have opened up an entirely new field of dating which promises to be of the utmost importance (it was, in fact, the subject of a resolution at the closing plenary session, which stressed the need for assistance in the promotion of this research). The success of his work has been due in large measure to Dr G. Bond's studies of the structure of wind-blown and water-deposited sands of Kalahari type. Soils transported into the Australopithecine caves include grains of sand (quartz) and chert (of dolomitic origin). It has been found that in modern soils in the area the quartz/chert ratio is constant; therefore, when this ratio is found to be different in the cave deposits, the cause is climatic. A higher proportion of quartz indicates drier conditions (for there is more wind transport and less solution of the dolomite). Brain has collected samples at 5 ft. intervals from vertical sections through the breccias at Swartkrans, Sterkfontein and Kromdraai (Makapan being partly alluvial is not applicable) and established the quartz/chert ratio of 500 grains in each case. 'Frosted' grains, which are characteristic of wind-blown sands as defined by Bond, are then counted and compared with the number of non-frosted quartz grains. To determine the degree of wetness or dryness of the climates the sand fractions in soils of modern dolomite areas of known rainfall and samples of breccia are similarly analyzed. Comparative diagrams will then show climatic conditions at the time of formation of the breccias with respect to areas of known rainfall.

Mr J. Robinson gave a clear summary of present knowledge of the Australopithecinae and exhibited a number of new specimens. From evidence of the spinal column and pelvis (as yet undescribed), supported by the position of the foramen magnum, it is now quite clear that the Australopithecines were fully erect. Another point to emerge from the study of limb bones, etc., is that they were very small creatures, less than 4 ft. high and weighing perhaps 40 to 50 lbs. (this would account for the comparatively small cranial capacity). 530 teeth have been described so far; it appears that the *Paranthropus* group were specialized for a vegetable diet, while the dentition of the earlier *Australopithecus* group indicates omnivorous habits. In both groups there was considerable sexual dimorphism. *Telanthropus*, of which there are now five specimens, seems to be close to the *Australopithecus* group. As Sir Wilfrid Le Gros Clark pointed out, the fact that at least two groups of Australopithecines were already well established during the early Pleistocene shows that the genus existed presumably during the Pliocene; whether the two groups are generically or specifically distinct does not matter very much at present. The unspecialized *Australopithecus* group is apparently earlier than the specialized *Paranthropus* (which Robinson described as a 'hominid gorilla').

THE FLORISBAD, HOPEFIELD AND TERNIFINE SITES. Dr A. C. Hoffman and Dr A. J. D. Meiring read papers on the Florisbad site, stressing the importance of 'chopper tools'. Dr Oakley, in a talk on the application of new analytical techniques on the dating of fossil skulls from African sites, said that the comparatively high nitrogen content of the Florisbad skull indicates that it comes from Peat 1. Nitrogen content of

bones from sands above and below the peat is negligible and it has been found that bone protein in peat or carbonaceous clay disappears very slowly in comparison with sand.

With respect to the Hopefield (Saldanha) site, it was found that the rate of fluorinization was very high and it appeared that all the fossil bones were broadly contemporary. This conclusion was supported by the results of uranium assay (although further determinations are required to test it statistically). Several speakers described different aspects of the work at Hopefield. Mr J. A. Mabbutt spoke of the physical background, linking geological features with climatic changes. The fossils occur in nodular calcrete, which descends coastwards to limestone ridges representing dried pan floors in which the bones accumulated. The site is transversely by ferricrete ridges, marking a preceding wetter period, and this ferricrete postdates the 20–25 ft. beach. The fossils accumulated during the decline of a wetter period, associated with regression of the sea during early Upper Pleistocene times. Mabbutt also described in another paper some Quaternary events in the winter rainfall area of Cape Province which provide a means of linking the climatic sequence to that of the summer rainfall area. Sea-level changes have been correlated with terraces of the Olifants, Great Berg and other rivers; it has been found that the terrace sequence of the lower Olifants parallels that of the Vaal. The importance of such work in correlations is obvious. Other papers on the Hopefield site were read by Dr R. Singer on the fauna and by Mr K. Jolly on the archaeology.

Professor C. Arambourg described his discoveries at Ternifine in Algeria, which are of the utmost interest. Early in 1954, two human mandibles were found associated with an Acheulean industry (at first described as Chellean) and mammalian fossils; the hominid remains were described as *Atlanthropus mauritanicus*. Recently a third mandible and a piece of parietal (not yet freed from its matrix) have come to light. At Sidi Abderrhaman in Morocco, M. Biberson has found another fragmentary mandible, associated with an evolved Acheulean industry, which seems identical with *Atlanthropus*. The dentition of *Atlanthropus* seems very similar to that of *Pithecanthropus pekinensis* and the two hominids may prove to be generically identical. Although Professor Arambourg exhibited casts of the jaws, he did not have specimens of the handaxes with him; from photographs it appeared that they had been flaked by the cylindrical hammer technique, and it is interesting to speculate whether this African *Pithecanthropus* was the maker of the industry, as seems probable.

OTHERS PAPERS. Amongst the other papers which cannot be described in detail here, some were outstanding and must be mentioned briefly. Dr H. B. S. Cooke described a new method of dating and correlation of Quaternary deposits and events in southern Africa, using industries to define time-lines, which was so detailed and complicated that it was impossible to digest in the short time available; it is hoped that he will publish his conclusions so that they may be discussed fully. He suggests that the glacial/pluvial relationship, though broadly true, suffers local modifications which may even lead to an apparent reversal of correlation. He believes that a detailed study of the present day complex climatic trends in the sub-tropical belt may lead to the resolution of these apparent anomalies.

Equally stimulating (and equally difficult to absorb in a short time) was Mr R. J. Mason's statistical approach to the analysis of post-Acheulean industries. Whereas the pioneer of this method, M. F. Bordes, distinguishes no less than 73 different tool types, Mason achieved simplification by sorting the artifacts according to their basic *shape* rather than their *use*. This should enable objective comparisons to be made by different workers in different areas, which has hitherto been the main stumbling block to M. Bordes' methods. So far Mason has only used his method within the Transvaal,

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and it will be interesting to see whether it is equally applicable in other fields. Briefly, he divides the artifacts into primary classes of two basic forms: quadrilateral and triangular. These are further subdivided into rectangular and square, isoscelene and equilateral. He has observed differences of behaviour of the groups in time from the evolutionary point of view. The primary classes are further grouped into secondarily prepared specimens, utilized specimens and plain specimens. Striking platforms are next divided into faceted, plain and indeterminate. Secondary classes include all artifacts that cannot be placed in the primary classes (cuboid forms, spheroids, etc.), but it is only the primary classes that are of major diagnostic and evolutionary significance. From the graphic expression of the behaviour of the primary classes, Mason has been able to distinguish thirteen separate industries in the M.S.A. of the Transvaal.

EXCURSIONS. The Southern Rhodesian excursion which preceded the congress was organized by R. Summers, G. Bond, C. K. Cooke and K. R. Robinson. The highlight was a visit to Zimbabwe, an unforgettable experience; and we were exceptionally fortunate in being accompanied by Dr Caton Thompson herself. The Khami ruins, a key site for the Iron Age in S. Rhodesia, are also most impressive. One day was spent in the beautiful Matopos hills, where we saw the painted rock shelters of Bambata and Nswatugi.

Our fears that the tight schedule fixed by our indefatigable Organizing Secretary for the Northern Rhodesian excursion might prove too much for the human frame to endure were quite unfounded. Dr Clark's time-table was so well planned that we were able to visit all the most important sites from one end of the territory to the other within a week. During the time we were in Livingstone we were able to study the succession in the vicinity of the Victoria Falls. At Broken Hill we saw the site where Rhodesian man was found and also a section through clays, above the mine workings, which yielded Hope Fountain, Acheulean, Sangoan and early M.S.A. industries in 1953. At Kalambo Falls, twenty miles north-east of Abercorn, five living floors have been exposed, ranging from final Chelles-Acheul to early M.S.A. The industries are associated with carbonized tree trunks, fragments of worked wood and peaty clays containing pollen (Clark, 1954). It is probably the most important site in the Territory and further excavations are to be undertaken shortly. At Twin Rivers Kopje near Lusaka, we were able to see the bone breccia of early Gamblian age excavated in 1953 (Oakley, 1954). Nachikufu cave, in the Muchinga hills, gave the complete sequence of L.S.A. industries in this part of the country (Clark, 1950) and, like the rather more impressive Nsalu cave, is decorated with schematic paintings.

The Katanga excursion, organized by Professor G. Mortelmans, was also an unqualified success. The richness of the material in this part of the Belgian Congo makes one regret the lack of excavations, which depend on the energy of part-time enthusiasts. The most important pebble-tool site is Mulundwa II, on the road from Kasenga to Elisabethville; the industry, which probably dates to early Kamasian times, appears to be a variant of the Kafuan (known locally as the Kaflian culture). The type site of the Katanga Acheulean (known here as the Kamoan culture) is at Kamoia bridge, 90 km. NW of Kolwezi. Working with picks, within a short time we had recovered some very fine handaxes and a magnificent chisel, characteristic of the succeeding Kalinian (Sangoan) forest culture. The industry at Kamoia bridge appears to be transitional between the Acheulean and Kalinian; we were able to form this opinion from a study of the collection made by M. Paul du Ry from this site. Other Kalinian sites were also visited in the region of Kolwezi. In the region of Kansenia, Dom Anciaux de Faveaux has discovered an industry which he has named the Kansanian (as yet unpublished); the

artifacts, which include uniface planes with steep fluting, look very fresh and may be quite late.* Father Anciaux has a most interesting collection of stone age industries at the Benedictine Mission at Kansenia, while at Kakontwe (west of Jadotville) he has numerous mammalian remains from a Gamblian bone breccia nearby: the finds included a human molar and a fine Stillbay-type point in quartz. At Kia 'N Tapo, 60 km. sw of Lubudi, is a profusion of schematic engravings and drilled holes on a vertical rock face outside a deep cave (Breuil and Mortelmans, 1952); probably they are of Bantu origin. Near Johnston Falls on the Luapula we saw many grooves for polishing axes; striations in the grooves show that a coarse abrasive was used.

Apart from the various individual collections we were able to see, studies in the Leopold II Museum (Dr F. Cabu's collections), in Elisabethville were of the utmost value. This applies equally to the National Museum of Bulawayo and to the lively modern Rhodes-Livingstone Museum at Livingstone. At the Victoria Falls, too, Dr Clark has made a field museum for the benefit of tourists, which will preserve for all time a section through the Zambezi gravels and their containing industries from Acheulean to L.S.A.

Our delightfully varied programme included visits to the Wankie Game Reserve, to several mines and to the vast copper and cobalt refinery at Jadotville, where we also saw the splendid collection of minerals (including uranium minerals) in the museum. During the congress in Livingstone, Dr Leakey staged an impressive demonstration of the manufacture of stone implements and then skinned a buck with some of them (using a pebble tool, a cleaver and a backed blade); headlines in a local paper announced: 'Dr Leakey skins buck with his teeth'!

We are immensely grateful for financial aid from the various governments, mining companies, etc., which enabled the cost of transport to be met; to the organizers of the excursions and their assistants; to kind hosts who gave us hospitality everywhere; and above all to our Organizing Secretary and his helpers. We enjoyed it all immensely and learned a great deal; our feelings can best be told in the words of the poet du Bellay:

Heureux qui, comme Ulysse, a fait un beau voyage,
Et puis est retourné, plein d'usage et raison.

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* Since the above was written, the Kansanian has been described by Dom A. Anciaux de Faveaux in 'Gisements et industries préhistoriques des hauts plateaux Katangais'. Brussels, *Acad. roy. des Sci. Coloniales, Mémoires*, Tom. II, fasc. 2.

Prehistory and Protohistory in France*

by GLYN DANIEL

IN November 1954 the French Prehistoric Society celebrated its fiftieth jubilee with a *séance solennelle extraordinaire* in the cinema of the Musée de L'Homme in Paris. The séance was presided over by the Abbé Breuil, doyen of French if not of all European prehistorians, and attended by most French prehistoric archaeologists of importance. Guy Gaudron, the Secretary General of the Society, gave a brief history of the society's inception and development; the Abbé Breuil spoke on 'La Perspective préhistorique et ses progrès'; messages of congratulation were sent to the Count Henri Begouen, who, with Gaudron, were made Honorary Presidents; messages were read from prehistorians and prehistoric organizations in other countries, and a small group of foreign representatives spoke in person—Sauter from Geneva, Kühn from Mainz, Frank Mitchell from Dublin, and Lacaille and myself from Great Britain.¹

In September 1955 the Prehistoric Society of Great Britain held its twenty-first anniversary meeting in Cambridge, an appropriate centre for a Society which grew out of the Prehistoric Society of East Anglia—itself founded in 1908, only four years after the French Prehistoric Society—which has owed so much in its development to Cambridge men like Grahame Clark and C. W. Phillips, and whose *Proceedings* are published in Cambridge at the University Museum of Archaeology and Ethnology.

These two jubilees provide an obvious moment for looking back at the development of prehistory in Britain and France in the last half-century, and more particularly the present position of prehistoric archaeology as an academic discipline and as general learning in these two countries at the present day. This comparison is made pointed by the appearance within the same year—1954—of two works on the French neolithic; the first, *Le Néolithique Occidental et le Chalcolithique en France*, by Professor Stuart Piggott, which, though it appeared as two articles in *L'Anthropologie*,² is really a short book, and the second, the Bailloud-Mieg de Boofzheim work already cited, is a book of 244 pages comprising 96 line illustrations. (This feature alone, in a French work on prehistory, is a delightful surprise and prejudices us favourably towards these two young authors). Of the first, the editor of *ANTIQUITY* has already said that it is 'a major contribution to European prehistory, fully illustrated by a sketch map of the pottery types and drawings of them, based on extensive travels and the visiting of museums in France, as well as on the printed sources'.³ Of the second, let me say at once that it seems to me the best piece of archaeological synthesis in the field of post-Palaeolithic and pre-Roman archaeological studies produced in France by a Frenchman—or rather two Frenchmen—since Joseph Déchelette, and how delightful that it should have the same publishers: Picard of

* A review of *Les Grandes Civilisations Préhistoriques de la France* (Paris, 1954) being volume 51, part 8, of the *Bull. Soc. Préh. Française*; and of G. Bailloud and P. Mieg de Boofzheim, *Les Civilisations Néolithiques de la France dans leur Contexte Européen* (Paris, 1955). 1900 francs.

¹ See *Bull. Soc. Préh. Française*, 1954, 497 ff.

² *L'Anthropologie*, LVII (1954), 401-43, and LVIII, 1-28.

³ *ANTIQUITY*, 1955, 64.

the Rue Bonaparte, who produced the *Manuel d'Archéologie Préhistorique Celtique et Gallo-Romaine*.

Why has one had to wait so long for a book like this Bailloud-Mieg de Boofzheim volume? The answers lie deep in the present state of French prehistoric and proto-historic research. The pages of ANTIQUITY have often contained sharp criticisms of French archaeology. One calls to mind many editorial notes, re-echoed in the editor's *Archaeology from the Field* and *Said and Done*, and Sir Mortimer Wheeler's memorable *This Fieldwork*,⁴ described in *L'Anthropologie* as a 'cruel satire'⁵ and his review of Leroi-Gourhan's *Manuel des Fouilles* which was translated and republished in French by Vaufrey in *L'Anthropologie*.⁶ It is not only foreign critics of French prehistoric archaeology who are vocal. Vaufrey in the pages of *L'Anthropologie* has been a constant critic,⁷ and many others in France have with great sadness felt the truth of this alleged national backwardness. In a recent article entitled 'Préhistoire, Protohistoire, Histoire', Professor J. J. Hatt of Strasbourg was forced to admit the 'retard de la science française dans le domaine de la protohistoire européenne' and added 'Il n'est que d'assister aux congrès préhistoriques internationaux pour en acquérir la triste certitude'.⁸

What do we really criticize in current French prehistoric archaeology? I, an ardent Francophile, who sometimes feels more at home in France than in England or Wales, would list six main reasons. First, with very few exceptions, the museums of archaeological materials are poor. Secondly, the apparent technique of excavation compares unfavourably with current apparent technique in the British Isles, Germany and Scandinavia. In the third place, field work, in the Williams Freeman-Crawford-Fox sense of regional surveys of antiquities from ground and air, is missing. Recently Crawford has said roundly that 'Fieldwork in the British sense has never existed in France'.⁹ Fourthly the utilization of complementary specializations for archaeological ends is not easily forthcoming in France, although the essays edited by Annette Laming under the title of *La Découverte du Passé* show progress in the right direction.¹⁰ Fifth, French Universities and French education in general give scant support to prehistoric archaeology; and lastly there is lacking in France a general interested public for archaeology—the sort of public whose interest in England can be tested by the sales of ANTIQUITY, the amount of archaeology that appears in *The Illustrated London News*, and the appreciation indexes of archaeological broadcasts and television programmes.

Before we discuss these criticisms we must remember two things. All these criticisms are over-simplified generalizations and they apply most to what we in Britain call the study of the Neolithic, Bronze and Early Iron Ages. In France the study of the Palaeolithic and of the Gallo-Romans has always been far ahead of the study of the three millennia that separate the Mesolithic from Julius Caesar. We are always being told that it is the richness of France in Palaeolithic and Gallo-Roman remains that is responsible

⁴ ANTIQUITY, 1932, 55.

⁵ *L'Anthropologie*, 1932, 658.

⁶ ANTIQUITY, 1950, 166.

⁷ See, for example vols. 49, 445-6, 50, 287-291, and 58, 564.

⁸ *Bull. Soc. Préh. Française*, 1954, 56.

⁹ *Archaeology from the Field* (1953), 208. Cf. *Said and Done* (1955), 301 and this sentence, 'The state of French archaeology is the greatest hindrance to the progress of knowledge in the sphere of European (and British) prehistory'.

¹⁰ It also was published by Picard, in 1952.

for the neglect of the intermediate periods, that in a country with Lascaux and Solutr  and the Pont du Gard, it is perhaps natural not to bother about Neolithic camps and Bronze Age urnfields. But France is extraordinarily rich in monuments of the Neolithic, Bronze and Early Iron Ages; why is it that Gavr'inis and the Marnian chariot-graves have received less attention than the Old Stone and the Roman Iron Age?

Some national difficulties lie behind this neglect, and the first is one of nomenclature. The French have tended to insist on using the word *pr histoire* to mean what Herzfeld called absolute prehistory and Grahame Clark has recently been calling primary prehistory, i.e. the study of the material remains of man from a time absolutely before writing was invented in the world. It is interesting that in the Abb  Breuil's presidential address to the jubilee meeting of the French Prehistoric Society, the prehistory whose perspective and progress he is describing is entirely the study of the Old Stone Age.¹¹

We in the British Isles use prehistory to mean all time before the Roman conquest, but most Frenchmen use the term *protohistoire* for the Neolithic, Bronze and Early Iron Ages—even D chelette had to distinguish *l'arch ologie celtique* from *l'arch ologie pr historique*—and it is French *protohistoire* that we criticize. It was D chelette himself who really created the study of French protohistory, building on the narrow classificatory systems envisaged by Gabriel de Mortillet and with a breadth of vision which only Cartailhac had before; but now, fifty years after D chelette, *protohistoire* is a poor relative in France of classical archaeology, a poor and despised relative. The first book under review, *Les Grandes Civilisations Pr historiques de la France—Livre Jubilaire de la Soci t  Pr historique Fran aise 1904-1954*, itself proves it: here in a book of 110 pages we find 80 devoted to the pre-Neolithic, 20 to the Neolithic and Chalcolithic, while a single article of ten pages (admittedly a most excellent one by Professor Hatt) deals with the seventeen immediately pre-Roman centuries of French archaeology. Hatt's article is significantly called 'De L'Age du Bronze   la Fin du L'Age de Fer; probl mes et perspectives de la protohistoire fran aise'. And even this stops short of the La T ne period; the beginning of the Mediterranean influences from Etruria and Greece in Gaul and Germany are the end of the *Livre Jubilaire*. Yet how odd seeing it bears on its cover, not a bison from the Salon Noir at Niaux, not even the charming dolmen with which the Chocolat Menier advertisements have for so long enlivened the pages of the Society's *Bulletin*, but the *krater* from Vix!

That is just the difficulty one appreciates in the current French idea of the early human past. Behind all this, behind the fact that the excellent little series *Que-sais-je?* produced by the Presses Universitaires de France has a book on the Palaeolithic and one on the Gauls but nothing in between,¹² is much more than the awkward French use of *protohistoire*. It is a feeling—deep-seated in French educationists and the public—that history produced from written sources and history produced from archaeological sources are essentially different; that history in France really begins with Massilia and Caesar, and that the story of man before then, whether it concerns that Camp de Chassey which D chelette so proudly called 'le pompeii de la France' or the sculptures at Angles-sur-l'Anglin, is *autre chose*, and anyway, almost geology. There are of course dying Gauls in the average Frenchman's imagery of his national past—and Vercingetorix and Druids,

¹¹ Not that the French themselves are consistent in their use of *pr histoire* and *protohistoire*. See the Paris volume (1935) of the *Congr s Arch ologique de France*.

¹² I refer to Lantier's *La Vie Pr historique* (1952) and Thevenot's *Les Gallo-Romains* (1948). Cf. in the same series Georges Daux's *Les  tapes de l'Arch ologie* (1942) with its great emphasis on classical archaeology.

but behind all that there is nothing in concrete imagery unless it be Darde's statue of prehistoric man on the terrace at Les Eyzies.¹³

In assessing the relative developments of protohistoric archaeology in Britain and France in the last fifty years we do sometimes forget how many potential scholars were killed in France in the carnage of the 1914-18 war, including the gallant Captain Déchelette. We forget too that in the fifteen years following the end of that war there occurred in Britain what can only be described as a major revolution in the technique, methods and aims of archaeology. Looking back at it from the middle of the century a quite fantastic gap separates the work of Munro and Windle and Rice Holmes from that of Fox, Wheeler, Crawford and Childe. We have never in England produced anything as good as Déchelette's *Manuel*—and the start made by R. A. S. Macalister in his *Textbook of European Archaeology* in 1921 was never continued—but France in the twenties and early thirties seemed quite unable to do anything to compare with, for example, *Wessex from the Air*, *The Archaeology of the Cambridge Region*, *Prehistoric and Roman Wales*, *The Dawn of European Civilisation*, *The Axe Age*, or the excavations at Verulamium, Lydney and Maiden Castle. It is only now that the fantastic gap we see in British archaeology is beginning to be closed in France—a quarter of a century or more after Fox and Wheeler were working in Wales and the Ordnance Survey had appointed its first Archaeology Officer and the University of Cambridge had set its first Honours Examination in Archaeology and Anthropology.

It is less important to analyze why what we call modern prehistory and the French *protohistoire* has taken so long to develop in 20th century France than to welcome the real beginnings of change which are taking place now. We in Britain were excessively slow to adopt the revolution in prehistoric method and technique which the Scandinavians organized in the second quarter of the 19th century. The French have been slow to adopt the revolution which the British largely organized in the second quarter of the 20th century. The men who are now working in France, men like Joffroy and Hatt, Arnal and Escalon de Fonton, Giot, Balsan, Vaufrey, Leroi-Gourhan, Patte and Borde—to mention, invidiously, a few names that come to mind—are beginning to restore to France the position in *protohistoire* which she lost when Déchelette was killed. It is therefore time in Britain that we began to give up our cruel satires and rumbustious fusillades and extended sympathetic understanding to the difficulties and problems of the new French archaeologists striving at the present day.

What is needed in France today from the point of view of the modern scientific study of the Neolithic, Bronze and Early Iron Ages? First, good museums, which means good responsible well-paid staffs with technical departments and laboratories, and new buildings. Above all St. Germain-en-Laye must become a real Museum of National Antiquities.¹⁴ In the second place there must be an improvement in the apparent technique of excavation in France, and I say apparent advisedly because the poor opinion of French excavation which many British express is largely based, not on visiting French

¹³ We see the same division of interest and neglect of *protohistoire* in the recently published Larousse *Histoire de France*. Leroi-Gourhan's chapter entitled 'La Préhistoire' goes from the beginning to L'Age du Bronze but the greater part of it deals with pre-Chassey times. The next chapter is 'La Gaule Celtique et Romaine', but only two pages are devoted to pre-Roman Gaul. Here, too, in this most modern synthesis of French history, the formative Neolithic, Bronze and Early Iron Ages have fallen between the two stools of the Old Stone Age and Rome.

¹⁴ Our own record in the British Museum since the war is a sad one, as discerning Frenchmen often point out. Lack of money is making our national Museum compare very unfavourably with, say, that at Copenhagen.

excavations, but on looking at the reports of French excavations in French publications, printed on poor paper with bad illustrations, badly drawn sections, and amateurish plans. Ten years of well-published excavation reports—because well-conducted excavations are going on in France—and this sneer would vanish; the reports in *Gallia* and the new *Revue d'Archéologie de L'Est* which Paul Lebel started at Dijon with such energy and good sense, are a beginning in the right direction, and so is René Joffroy's *Le Tresor de Vix*.¹⁵

The third requirement is field work, and by this I mean regional field studies of a complete nature well published with photographs, maps and plans. There have always been French monographs of a regional kind—to mention only a few, Paul du Chatellier's *Les époques préhistoriques et gauloises dans le Finistère* (1907), Dubreuil-Chambardel's *La Touraine préhistorique* (1923), and Louis's *Le Gard préhistorique* (1932), but they belong to the old style of field work which collects and lists rather than presents a synthesis based on regional field work. Gabrielle Fabre's *Les Civilisations Protohistoriques de l'Aquitaine* (Picard, 1952) is the most recent attempt at a regional survey in France, but it is poorly illustrated and with most inadequate maps. I shall not feel happy about the resurgence of French protohistoric studies until something like Crawford's *Long Barrows of the Cotswolds* or Hencken's *Archaeology of Cornwall and Scilly* or Curwen's *Archaeology of Sussex* has been produced for a French department or *pays*. Of course one of the real reasons behind the backwardness of French field studies is the lack of awareness of types of field monuments; even Déchelette hardly mentions the shape of barrows, and those who still rely on him or Cartailhac for a knowledge of French chambered tombs may be pardoned for thinking there are no long barrows in France. If only there were some French equivalent to the Ordnance Survey's *Field Archaeology* or Ó Ríordáin's *Antiquities of the Irish Countryside*, a real awakening of field studies might come about.

In the end, it comes back to two things—money and men. Far more money must be spent in France on museums and Government archaeological services and archaeologists in Universities. At present there are Professors of Prehistoric Archaeology in Bordeaux, Toulouse and Strasbourg, and nowhere else¹⁶; to me it is almost unbelievable that great Universities set in areas of rich archaeological materials like Rennes and Caen, Poitiers, Lyon, Aix-Marseille and Montpellier are without Chairs of Archaeology, and the absence of teaching prehistoric archaeologists on a professorial level in the great University of Paris is flabbergasting. How the ghosts of Lartet and Gabriel de Mortillet and Bertrand and Déchelette must chatter in rage together as they contemplate the sad state of academic prehistoric archaeology in France! There are not enough men to fill these posts, we are sometimes told: the whole process of development will be slow. The answer is, train the men. The French Government through its *Centre Nationale des Recherches Scientifiques* and its *Direction des Relations Culturelles* should, for the next ten years, have external bursaries and travelling scholarships so that promising French archaeologists could spend from one to three years working in England, Ireland and the Scandinavian countries. There will never be enough money, we are also told. The cost of real research is often exaggerated. Who, asks Vaufray, in his stirring appreciation of Professor Piggott's work on the French neolithic, would have paid in France for anyone

¹⁵ *Presses Universitaires de France*, Paris, 1955.

¹⁶ I refer to Nougier at Toulouse, Malvesin-Fabre at Bordeaux and Hatt at Strasbourg, but I am fully aware that Hatt is Professor of more than prehistory; he, like the Museum at St. Germain-en-Laye is concerned with *Antiquités Nationales*. Perhaps we in Britain might begin to have Professorships of our National Antiquities?

to do the field work and visits to Museums which Piggott did ? In this rhetorical question we perhaps see some of the same defeatism met when a prehistoric archaeologist in one area of France will tell you that he can never for financial reasons visit other far distant areas of France. I have confirmed with Stuart Piggott that the costs in France of the field work and museum study that went to make up his neolithic studies were no more than £100 to £125—and I am sure that included some good meals and a good many glasses of wine.

It is not, in the last resort, money or men that are lacking, but an attitude of mind. We in Britain had little money and less men in 1920, but those men had a sense of high purpose. They achieved work which persuaded intelligent administrators and guardians of public purses to pour out money for archaeology, and were able to get their work understood and appreciated by the general public. There is in France at the moment a group of people with the same high purpose ; let us hope they will persuade French administrators and guardians of public purses to spend money on archaeology. That is why we welcome so warmly the Bailloud-Mieg de Boofzheim book ; it can be criticized in detail—what book cannot ?—but it does set the French neolithic and chalcolithic cultures fairly in their European setting, and it is written by Frenchmen in French. It should be read by all the seventeen Rectors of the French Universities, by all the Prefects of Departments, and everyone in Paris who is remotely concerned with the past of France. The splendid second millennium in the French past will then begin to live for them.

But let us hope too that the new French protohistorians will get their work through to the general public as well, for without the support of peasant farmer and landlord, of *instituteur* and priest, we shall not readily see the day when great megalithic monuments are not destroyed by road menders or used as bicycle sheds, when antiquities are marked clearly on good maps, when the incomparably rich protohistoric past of France is studied and loved as part of their national heritage by the whole French nation. We already have a statue of Boucher de Perthes at Abbeville ; the day may not be far distant when a statue of Joseph Déchelette will look out over the factories and cafés of Roanne.

The Smaller House and its Significance

by H. BAGENAL

MONMOUTHSHIRE HOUSES. A Study of Building Techniques and Smaller House-plans in the 15th to 17th centuries. Part II. Sub-Medieval Houses c. 1550-1610. By SIR CYRIL FOX and LORD RAGLAN. Published by the National Museum of Wales, Welsh Folk Museum, Cardiff. 1953, pp. 135, 21 plates, 52 text figures, 3 maps, price 17s 6d. Also Part III, 1954, pp. 178, 31 plates, 72 text figures, 2 maps, price 21s.

WHAT was the character of the early farm house? On what scale did it develop? What culture did it achieve? What is its place in the history of building? Many students who are well informed about manor-house and college, and royal palace, may not have envisaged that other *scena*—the world of the cotter and yeoman and small freehold farmer. Yet this humbler world with its little 'hall houses', its sleeping lofts, its casually placed kitchens, its dairies, granaries, and ox-houses, underlay that other heraldic world and greatly contributed to it. Moreover it is embedded in our literature to be discovered by anyone who looks for it. And today it makes a special contribution. Building studies in all periods have been hypnotized by the glare of the great styles. This is quite natural. But the result is that the structural principles of the elder world have been masked by dome and vault and buttress systems, by highly developed masonry, and specially by the elaborate techniques of framed and jointed carpentry. But now those early structural principles are coming into view. We find them in the new studies of hut circles, in archaic temple models, in gavel-fork and cruck cottage, in old hearth complexes, in long-house plans, in Celtic building laws. All this belongs to vernacular building and is the heritage of the smaller house. Again nothing is more interesting than a vernacular style evolving its own humanism: and to find this we must look to country building as it developed here and there, with fertile limits set and in seclusion but not isolation.

In this we are already indebted to Welsh scholarship. And now Parts II and III of *Monmouthshire Houses* by Fox and Raglan, have recently been added to Part I to make a remarkable work which draws up a curtain on the whole subject. It originated in an aesthetic impulse, namely to preserve the beautiful farm house known as Upper Wern-hir in Llanbadoc parish near Usk, destroyed during the war 'for security reasons'. The house is presented in Part II in careful plans and sections made by Fox. In our FIG. 1 we are privileged to give the cross section from II, FIG. 4a. We are told in Part I, 'The survey was started in order to discover the origin and history of a style of building—stone-walled, with oaken doorways, and unglazed oaken windows exemplified in Upper Wern-hir. . . . The range of the enquiry was extended in the course of the investigation to cover the problems presented by small houses of character in the Monmouthshire countryside'.

Briefly the result of the survey was to verify a 'Regional style' of considerable interest, in which, owing to seclusion, we can see today a picture of the forgotten, oak-windowed, unglazed, house of considerable size: in addition we can discover behind its plan the cruck house of primitive times developed to a final peak before its disappearance; and thirdly we can follow the 'Regional style' forward through the 17th century and watch it developing its own humanism. The authors speak of this latter phase as the

'Renaissance period' yet it is greatly undisturbed by the rhythms of Italian or Dutch art. There is the main picture. But the authors discerned two other things; a central

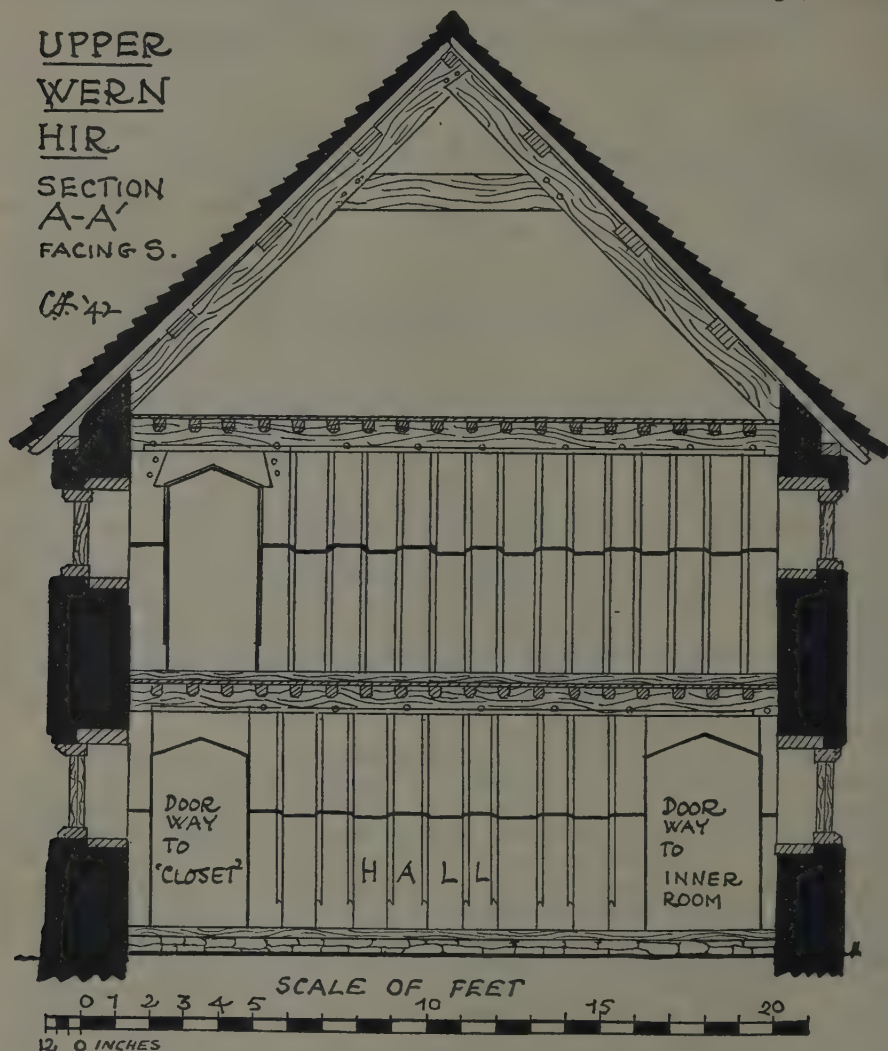


FIG. 1. UPPER WERN-HIR. CROSS SECTION
National Museum of Wales
From a drawing by Fox, 1942

economic problem, and that a good picture is given by their house plans of the social revolution of the Elizabethan period. It is in fact the relating of these aspects with the

techniques of building which give the book its special interest. In the late 16th and early 17th century 'rural Monmouthshire was a land of gentlemen farmers, many of whom bore arms, served as high sheriff, and sent their sons to the Universities and Inns of Court. How they got the wherewithal to do these things, and to build houses often too large for their modern successors, from their small arable farms, remains obscure'. (I, 13). The field evidence was that the Monmouthshire countryside was transformed by re-building and by new building between about 1550 and 1610. In a tract 20 by 14 miles in area, some 144 farm houses show features which illustrate this Regional Style. Now the rise to wealth of the yeoman farmer is a great theme in the literature of the period. Harrison, Sir Thomas Smith, and the author of the 'Discourse of the Common Weal' particularly mention it. Our authors link it with the rise in wheat prices and in Part II quote Professor Victor Morgan's conclusions on the subject (1950) and give his chart of wheat prices. The steep rise begins in 1530, and from the combined evidence of research in the field, and of the economic data, they place the beginning of their Regional Style at about 1550. And these new houses were unglazed: measured drawings of the oak windows with close set diamond shaped mullions are given (III, FIG. 20 a) and no evidence was found of calms with horn panels. Then follows an interesting comment. 'Privacy our farmers attained in the Regional phase 1560-1600 but the cruel draughts from the glass-less windows, inadequately shuttered, continued; these must have tried the hardest of folk and shortened the lives of many. The age of the (comparatively) draught-less house, that is the age of glass cheap enough to be within the means of anybody able to build a good house was delayed until the turn of the century (in 1599 as far as we know). This represents a permanent amelioration in the conditions of life—an improvement . . . to be compared only with the elimination of the smoke-filled rooms when gable fireplaces and chimneys came to be built'. We are here at the centre of the social aspect of these studies. The early plans show a remarkable tallying of unglazed windows opposite each other: they were shuttered: and the authors point to the necessity of closing the shutters on the 'weather' side and getting light from the sheltered side. Also opposite windows and opposite doors was a means of controlling the fire. But this meant that plans must remain of the simplest. Hence glazing, by overcoming this necessity, freed walling space and enabled plans to develop through the 17th century. Also the wider spacing of window mullions led to the need for wrought iron bars. Here I would link up Fox's drawings of window development with a curious story in Thomas Harman's *Caveat* (1567)—'Thou seest' says one rogue to another 'that this house is stone-walled about, and that we cannot well break in, in any part thereof; thou seest also that the windows be thick of mullion that there is no creeping in between'. These are the 'sturdy beggars' who, in Elizabethan history represent economically the opposite side of the shield to the wealth of the free-holders. Thousands were dispossessed by the enclosures and by the sheep farms. The activities of a class of beggars known as 'hookers' are described by Harman. They reconnoitred the windows by day when asking for charity, and returned at night with a species of fishing rod and made a trade in stolen fabrics, like small table carpets.

In these houses we find that the plans still derive from the medieval hall-house but that there are now fireplaces and separate rooms on upper floors. The desire both for privacy, and for freedom from smoke, were the motives and they led to a development, the steps of which can be well studied in this book. Between Chaucer's time and Shakespeare's, something remarkable happened in social history and it can be summed up in the word *bedrooms*. In Chaucer's time there is privacy out of doors in green-wood but not indoors in the hall-house with, at best, one end screened off with a 'travers' curtain,

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and a ladder leading to a half-loft in the roof. The Reeves Tale hinges on the fact that the miller's guests at Trumpington had to share the same sleeping chamber with the family. 'There was no roumer herbours (roomier quarters) in the place' says the

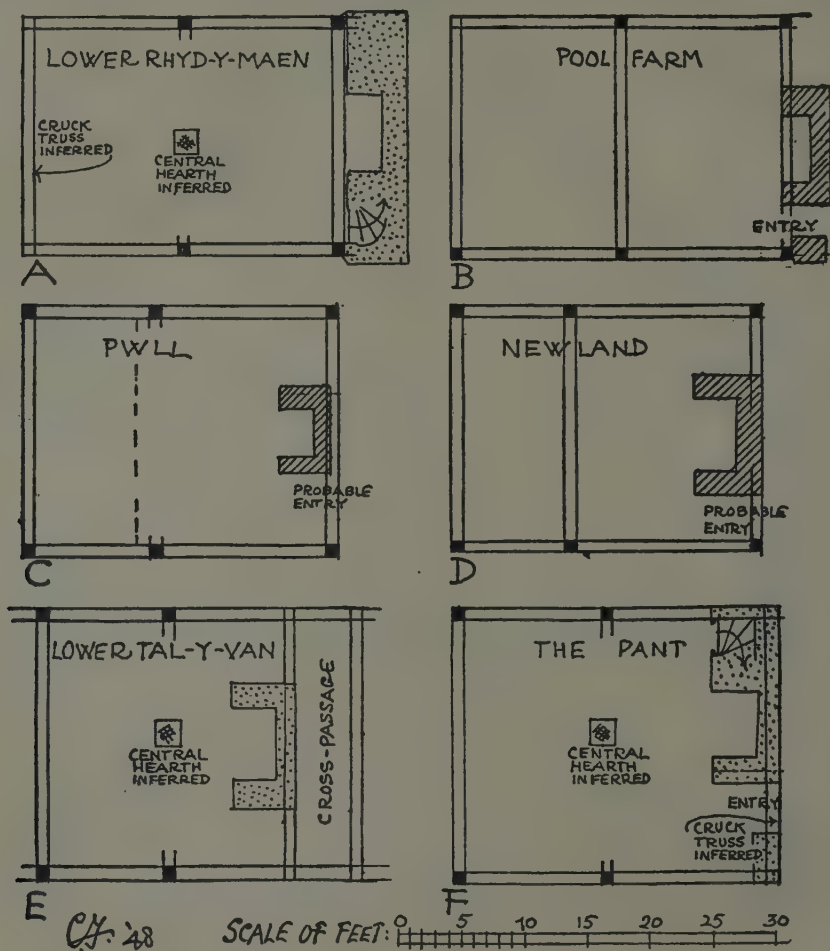


FIG. 2. SCALE DIAGRAM OF SIX CRUCK-TRUSSED HOUSES OR HALLS, EACH OF TWO BAYS
Fireplaces in B, C and D are held to be contemporary; in A, E and F to replace central hearths
National Museum of Wales

narrator. Hospitality and restricted accommodation combined to convert the hall-house into something better by making use of the roof space. But once floored over, the smoke could not rise into the roof space, and so became more of a nuisance. Smoke had advantages then, as it still has in African huts: a coat of 'Stockholm tar' soon covers and

preserves the timbers, there is protection from insects; and in early times the smoking of hams, joints, eels, and hog puddings, could be continuous. In the Saxon and Gelderland *bauernhaus* (boors' or peasants' house) the smoke from the hearth on the paved dwelling area curling up into the gables and out through the end gablets continued well into the 18th century.¹ But there was always the risk of fire in thatched buildings, and there was in England the example of the wealthier halls with their reredoses and canopies over the hearth. Hence the adding of stone stacks. And one sees how an access of rural wealth easily accounts for the great increase of chimneys noted by Harrison in 1587. The development of the cruck-roofed house in response to these movements is clearly analyzed by Fox and Raglan in their interesting diagram of early plans divided into classes (1, FIG. 21) which through the courtesy of the publishers we show in our FIG. 2. The evidence is carefully given from the buildings themselves. The gable fireplaces in A, E, and F probably replaced earlier central hearths. The remainder are held to be contemporary. 'We can fairly claim that these stacks, when not contemporary, were added before the upper floors in these houses had been inserted. In other words the change over from the smoke filled interior of the Middle Ages to the comparative comfort of a smokeless room . . . took place while a significant medieval social custom, that of living in a hall open to the roof was still current'. These plans also show that the stone stack on the gable end, with entrance door beside it, and a central pair of crucks open to the roof was the common form of early house in Monmouthshire. An exception occurs in the case of a little parsonage house called 'Church House' in the parish of Kemeys Commander which has a contemporary floor in a cruck roof—the small size of these priests' dwellings is an interesting social fact'. (1, 48).

The next step was the inserting of true floors (as distinguished from half lofts) in the late 16th century. This required something better than ladder access, and we are shown stone steps forming part of the massive stone stack. This in turn led to the freer use of the upper rooms. So we are shown that the revolution of the fireplace contributed to the development of bedrooms. But it is likely that the new rooms had variable uses—'beds in a store-room, stores in a bedroom' as our authors remark; and one recalls that Thos. Tusser in his August husbandry tells his maids to gather mustard seed and lay it up 'sweet in the sollar'.

Designing for privacy can likewise be followed on plan in the early screening off the end of the hall by a wooden partition to form a 'parlour', which I have suggested may derive from Chaucer's 'travers'. This gives the 'two room plan' emphasized by the authors as fundamental to Monmouthshire planning. Also they show how this family stronghold (as at Wern-hir) was sometimes bolted and barred against the cross passage and its traffic—a house within a house'. The plan type suggests that the 'shippon' or byre could be re-placed easily by 'the third room'.

The medieval background of some 37 cruck buildings is of course of great interest in itself, and has been discussed by James Walton in his review of Part I (ANTIQUITY, xxvi, 1952, p. 49). I would only point here to the lightness in weight of the Monmouthshire cruck. In country building, as distinguished from guild building, the quick raising of the roof principals, from a purchase on the ground, and assisted by neighbours, was convenient in the building of a new house. The thinning of these regional crucks

¹ The 'blue house' at the Netherland Folk Museum, removed to Arnhem in this century is said to have been in use without flues up to the time of its removal. For a thorough study with plans of this type see Lindner, W. *Das Niedersächsische Bauernhaus in Deutschland und Holland*, Ernest Geibel. Hanover, 1912.

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(we give in FIG. 3 the authors' section of Pool Farm I, FIG. 2) probably had some such object, and would have facilitated sled transport before road paving. But the mode did

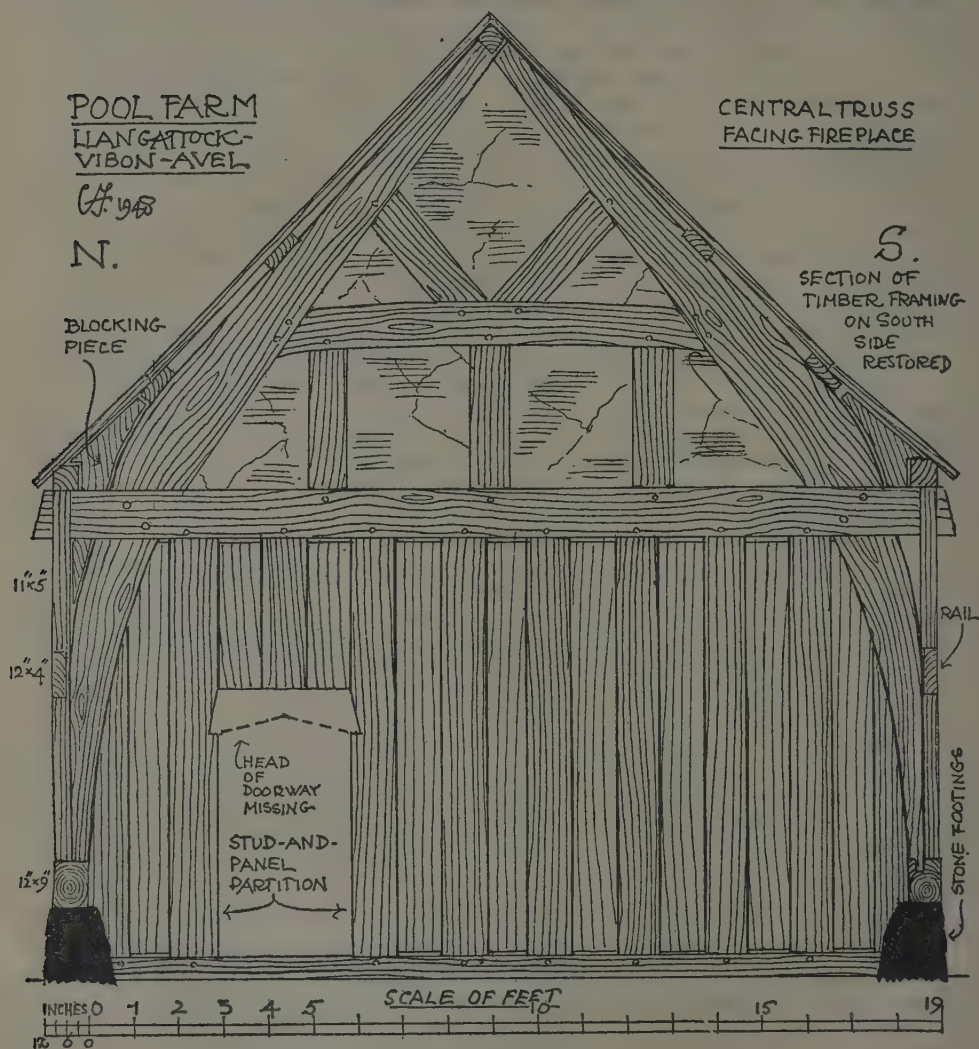


FIG. 3. LLANGATTOCK VIBON-AVEL. POOL FARM—SECTION

National Museum of Wales

From a drawing by Fox, 1948

not give much headroom for the inserting of floors, hence the various attempted improvements like 'upper crucks'. This suggests that crucks remain an intimate part of the

hall house. Also crucks illustrate the old structural principle of supporting the roof at the apex; the ridge piece and not the wall plate was thought of as carrying the load. This is an entirely different structural principle to that found in developed roofing, and accounts for the mythology associated with *rig-tree* and *nen-pren* and centre post. In Yorkshire early purlins are known as side trees; the width of bay was given by heavy roof-tree and side-trees and reached 15 ft. But we find in Monmouthshire a lighter carpentry altogether: the ridge is relatively small and purlins appear to be cut planks giving an average width of bay very much less. I disagree with the authors in calling these purlins 'heavy'. We appear to have in Monmouthshire a mode looking back a thousand years but which here comes to a high refinement in a school of its own.

The simple two-bay cruck dwelling was probably not an isolated unit but the centre of a group of related buildings. We find in Chaucer's *Tale of the Wife of Bath*, the statement that holy friars, in order to exorcise fairies, take it upon themselves to bless—'halls, chambres, kitchenes, boures . . . bernes, shipnes, dayeryes'. This seems to confirm our authors remark that each such unitary house 'must surely have been the important centre of the family, the fire-hearth, amid a group of less well finished household structures, a kitchen certainly as well as a room for spinning and other women's work'.

The location of the medieval kitchen has often puzzled students. There seems no rule as to its position, and the hall does not later become the kitchen. Fox and Raglan give detailed information about a number of separated kitchens with their distances from the house. That at Ty-mawr, Dingestow, is in use to this day. Also there is a clue in Fox's plan of Nant-y-Bannw: it shows a kitchen not separate, but located at the back, and having steps leading down to a stream. It was clearly convenient that kitchen and dairy should be near the water supply and this might or might not lead to separate siting. At Cholwich Town on Dartmoor dating from the 13th century, the stream still flows through the old dairy. The student should note that old farm lay-outs are given in this book in vol. II, pp. 79 and 99.

Now the planning for farm economy illustrated in this book gives us I believe a clue for distinguishing farm house from manor house. The authors give an interesting discussion (I, p. 88) of the traces of 'screens' and 'spere truss' characteristic of the manor house in a few of their earlier buildings; and it is clear that the manor house plan, satisfying manor house requirements, was an active influence in Wales and the Marches. But I believe the long-house plan, with opposite doors and cross *passage-way*, is older and more significant. There we find the farm centre, the junction of dwelling and shippon, giving access to family, herdsmen, and beasts. The need for beasts to be easily accessible for feeding and milking; and easily attended for calving at any hour of the day or night is the age-old governing fact. Where dairying and cheese-making remain of first importance, as in Holland, the beasts remain under the same roof to this day in modern buildings. On Dartmoor there are still long-house farms in use, as at Hole. This is the rationale both of 'long-house' and 'bauernhaus' plan.² But a shift of emphasis in mixed farming to arable, means fewer beasts, and more granaries and barns, which with their threshing floors can be wholly separate. Then shippon can be well used for 'service'. The plan

² There is also a modern application. When the close attention to stock by peasant owners in a severe climate breaks down, then the quality of the breed and the quantity of milk and butter and eggs for public supply is liable to decline: this seems to be the result of collective farming in Russia, and one of the causes of the agricultural crisis.

(FIG. 4) which we give of Wolvesnewton (III, FIG. 27) illustrates the persistence of the long-house plan and the re-placement of shippon by service room. A factor always present in all rural economy is the problem of where son and daughter-in-law and grandchildren are to live. From all this there is strong reason both for 'third room' instead of shippon, and also for adding granaries. This can be studied in the interesting lay-out of Lower Tal-y-van, Dingestow, where a new granary is seen added to an old barn (III, FIG. 37).

All this seems to show that an early pastoral economy with its powerful parental forms had been succeeded by an arable economy which has brought wealth, and has altered but has not broken the old forms. It accounts for the small shippons, and the absence of dairies. Yet another origin can be read in this book namely the development of the common L plan when the kitchen becomes attached to the rear of 'Service': this can be surmised from the plan (FIG. 5) of Kingsfield, Grosmont (III, FIG. 9); it is shown complete in the plan of New House, Llangibby, 1700 (III, FIG. 63).

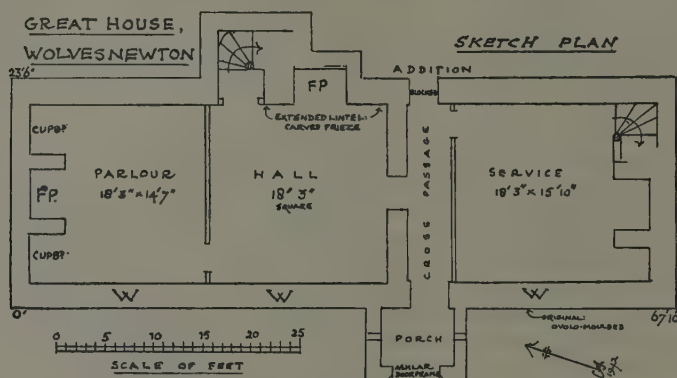


FIG. 4. PLAN OF GREAT HOUSE, WOLVESNEWTON
National Museum of Wales

When we come to assessing the artistic value of a vernacular style we must face the difficult task of seeking comparisons and setting standards. Yet it seems desirable, in profound studies of this kind, because they are means to the understanding of history and of art. We have in England two domestic styles which show aesthetic completeness; namely the Cotswold stone style and the 17th century Yorkshire gritstone. The Cotswold, using a small stone slate has multiplied its gables and kept its roof roughly at the early thatch pitch higher than 45° . It achieves a rather insipid harmony of stone roof, and wall, and window mullion, which yet exerts an enduring Gothic charm. In the Yorkshire farm house with its wool store the York stone slates brought the pitch right down, and the wide gables, often with a central window mullion carried up to a roof finial (as at Riddlesden near Keighley) confer a massive Dorian character which becomes later a heavy classic, but for a period in the 17th century stands poised on itself—a unique masonry style. But this was partly due to the richness of the gritstone supplies which easily gave deep lintels, jambs, bond-stones, paving slabs, and every kind of walling. Such are some of the artistic standards set. In Monmouthshire the materials are limited. The roof pitch (except for a single medieval

THE SMALLER HOUSE AND ITS SIGNIFICANCE

example of high pitch) appears to remain roughly at 45° at all periods whether for thatch, stone slates, or tiles. This pitch does not give aesthetic character: it is neither 'high' nor 'low'. To understand the Monmouthshire *quality* we must distinguish between

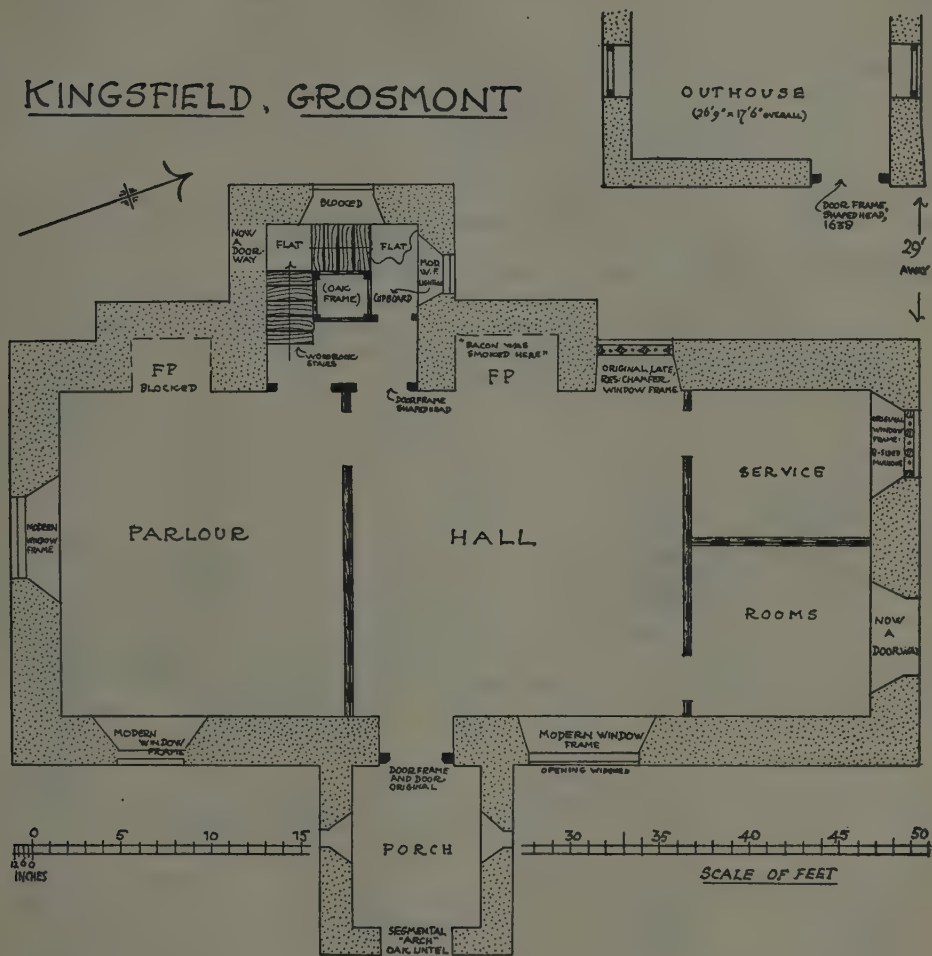
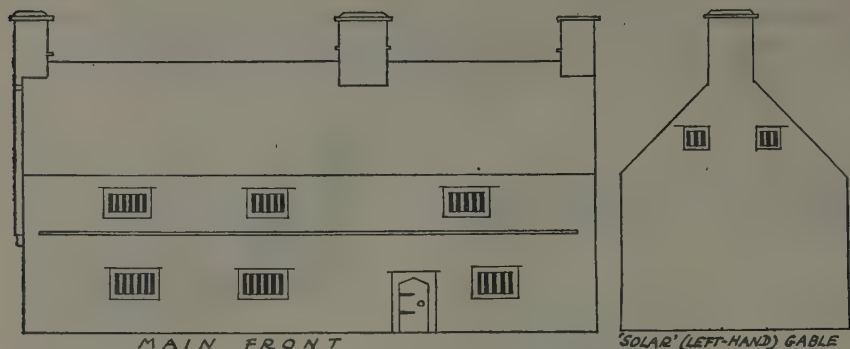


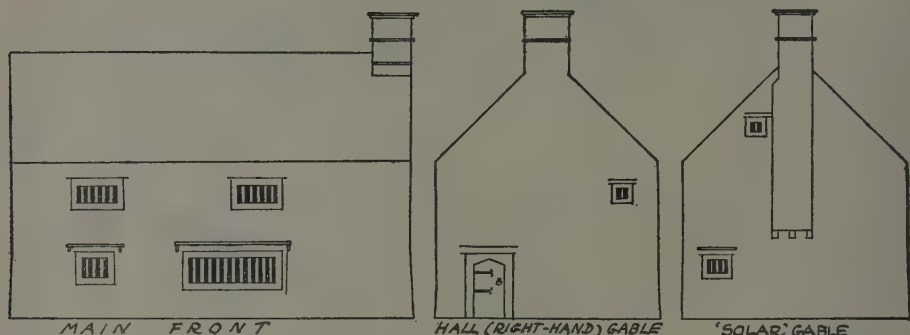
FIG. 5. KINGSFIELD, GROSMONT. PLAN
National Museum of Wales

waller and mason. The region appears to have plentiful rubble but not bonders and lintels to be compared to Yorkshire or Cornwall. But timber from reclaimed forest land remained plentiful. Hence waller and carpenter were the operative craftsmen rather

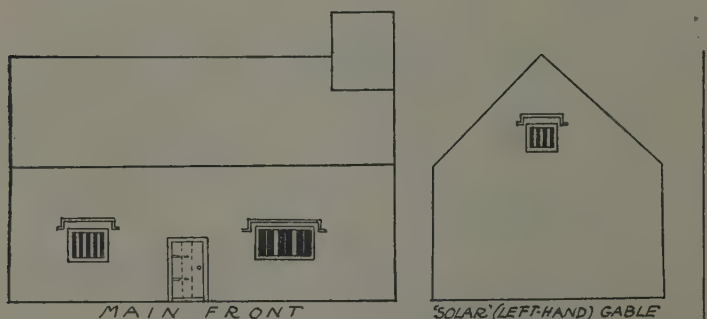
THE 'REGIONAL STYLE': DIAGRAMS OF HOUSE-TYPES



TYPE III HOUSE : THREE GROUND-FLOOR ROOMS



TYPE II (A) HOUSE : TWO GROUND-FLOOR ROOMS



TYPE II (B) HOUSE : ATTIC BEDROOMS

THE REGIONAL STYLE BEING UNIFORM, AND THE TYPES A MATTER OF SIZE ONLY, VARIANT DETAILS ARE DISTRIBUTED AS CONVENIENT IN THE DRAWINGS. THE EXTREME RANGE OF WINDOW BREADTH IS ILLUSTRATED IN TYPE II (A)

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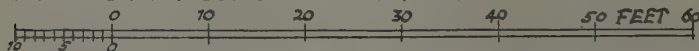


FIG. 6. HOUSE TYPES IN THE MONMOUTHSHIRE REGIONAL STYLE

than the mason.³ The lack of freestone in Monmouthshire meant that the early diamond oak mullions were not replaced wholesale by stone mullions, but were themselves developed in strength and lintel size, and spacing, to match the heavy walling. The authors sum up the technique in the phrase 'stone for walls, but not for windows'. The windows retain their useful drip moulds above them; and mullions were moulded before they were glazed. (Yet owing to cross fertilization the early mouldings imitated masonry). Artistically then the style is marked externally by massive rubble walls, slightly battered, in which are set the heavy oak windows (FIG. 6). Strings, and pent-house roofs over the doors, and huge stacks add to the character. The development sprang chiefly from its internal disciplines. A large porch was added to the cross passage as at Wolvesnewton (see plan) and it was natural then to seek for a symmetry and develop a 'front'. But the H plan does not seem to develop, nor the interesting English groupings that belong artistically to the H plan. The parlour becomes as large as the hall and needs a fireplace; and an enlarged wood staircase is found placed on the flanks, between two fireplaces, as at Kingsfield, Grosmont: see plan (FIG. 5 above). The enriching of the oak beams in the room by mouldings and ingenious stops is refined and original. Whether we turn to Fox's beautiful plans and drawings, or to the finely executed sheets of details by Mr Leonard Munroe, one might say that, to the initiated, the architecture is set down like a musical score for secret reading. The developed style does not modulate into a heavy Palladian, but seems to become, towards the end of the century, part of that first rational classic which, with *croisé* windows and high roof, was taken by Webb and Pratt as a prototype. It is seen, in the Welsh mode and still on the hall plan, in the photograph at the end of Part III of New House, Llangibby.

In conclusion a regional survey of such completeness forces upon us an urgent issue. It shows us what can still be learned, if we should record what is left of vernacular building and place it effectively beside the new studies in local history. We have to classify roughly our old agricultural regions and relate farm-house planning to the farming. Above all we have to record, *before it is too late*, the surviving plans. An old farm out-building long decayed, is pulled down unrecorded because no one sees that once it was the dwelling itself. A cottage goes here, a barn there, a house is altered beyond recognition. And we have in England no national central folk museum like Cardiff or Skansen or Arnhem. While we delay it is the monuments themselves which are disappearing.

We wish to thank the National Museum of Wales (the publishers)
for the loan of the blocks illustrating this article. EDITOR.

³ In Cornwall and Devon the waller's craft goes back to old 'cyclopean' techniques, and tends to be an extension from field clearing and field walling to farm building generally. Our authors do not clearly inform us whether their Monmouthshire footing walls, and battered walls, are primitive in origin or derived from medieval guild building.

A Bronze Sword from Ugarit with cartouche of Mineptah (Ras Shamra, Syria)

by CLAUDE F. A. SCHAEFFER

Collège de France

DURING the seventeenth campaign of the Ras Shamra excavations (1953), we uncovered on the east side of the palace a block of private houses between two streets running parallel in an east-west direction. In one of the buildings (which is still not completely excavated) we found several bronze objects buried in a corner of the inner court in the middle of which were two troughs. Stratigraphically the building is assigned to the latest layer of Level I which dates from the 13th century B.C. Amongst the ceramic remains found¹ is a female Mycenaean idol of poor workmanship and a bowl with wishbone-handle of Cypriot type, also plainly of late date.²

The sword was found at Point 912 at a depth of 1.50 m., a little below the ancient surface of the court. It is of importance both for Ugarit and for the general history of the 2nd millennium; I therefore have decided to publish an account of it before the conclusion of excavations in that quarter. The sword is in mint condition, so to speak, and has a fine green patina; it is 74 cm. long including the hilt (15 cm.). The blade is 5 cm. wide at the hilt end and has both edges unsharpened. It could be used both for cutting and thrusting. Down the middle are three grooves uniting at the point, and at the other end is the cartouche of the Pharaoh Mineptah (PLATE I), his name preceded according to my colleague, J. Vandier, by the epithet 'master of crowns' which is usually found in front of the royal protocol. The cartouche is 30 mm. long. In spite of the simplification required for engraving hieroglyphs on bronze, the character of the signs has been so well reproduced that one must suppose the engraver to have had some experience.

The discoveries at Ras Shamra have shown that from the time of the Middle Empire representatives of the Egyptian court and private persons coming from Egypt lived at Ugarit,³ and that there were relations between the two lands throughout the duration of the New Empire down to the last period, that is to say, until the second half of the 13th century. An inscription found during the 18th campaign (1954) in the tablet-kiln of the palace, which dates from the final period of Ugarit, mentions the arrival from Egypt of a man named Abraham.⁴ We may therefore suppose that one such person guided the hand of the engraver of our cartouche.

The sword is not of an Egyptian type. It is known that these big swords did not form part of the armament of Egyptian soldiers till the 13th century when Ramses II and especially his thirteenth son and successor, Mineptah,⁵ began enlisting quite important

¹ For these discoveries see my *Ugaritica III*, in the press. The same volume contains an account of two other bronze swords from Ugarit.

² It may be compared with that illustrated in *Ugaritica II*, fig. 121 (18).

³ For evidence of this see the Middle and New Empire Egyptian steles found at Ugarit *Ugaritica I*, pp. 20, 39.

⁴ See *C.R.A.I.* 1955, séance du 11 février.

⁵ Whose reign is dated 1234-24 by Drioton and Vandier, and 1224-1204 by Rowton.



SWORD OF MINEPTAH FROM UGARIT (RAS SHAMRA)
length 74 cm. (2 ft. 5 ins.)

bands of foreign mercenaries of Libyan, Asiatic and also European origin.⁶ The long sword is a type of weapon that originated in Asia Minor and in the East Mediterranean region. The oldest types of tanged swords hitherto known were found during the Turkish excavations at Alaca Höyük in the middle of Asia Minor where they go back to the end of the 3rd millennium.⁷ Thence the type spread to proto-historic Europe where it was markedly developed from the middle of the second millennium⁸—a fact which certain archaeologists, attracted by theories of nordic priorities, have used to support a claim for a European and particularly a German origin for it.⁹

The discovery of a tanged sword dating from the time of Mineptah at Ugarit, where we must suppose it was made,¹⁰ strengthens the view that the type was of Mediterranean origin. It might even be tentatively suggested that Mineptah had ordered from Ugarit swords of this type, marked with his cartouche, to arm the auxiliary troops who were to resist the joint invasion of Lybians and contingents of Peoples of the Sea at the famous battle of Perir in the fifth year of his reign (about 1230 B.C. or 1220 on the short chronology.¹¹ The Karnak inscription celebrating the Egyptian victory begins in fact by describing the armament preparations made by order of the Pharaoh,¹² a fact suggesting that, as happens so often with great war-leaders, his success was due as much to detailed organization as to the strategic skill of the commander or the courage of his soldiers. For the rest, the discovery at El Kantara in the Delta¹³ of a sword comparable with that of Mineptah shows that this type of weapon was actually used in Egypt at the end of the 13th century.

The sword with Mineptah's cartouche is the latest find at present known from Ras Shamra that indicates relations between Ugarit and Egypt. It would seem then that the Akawash, Tursha, Luki, Shardanes, Shakaresh and other 'peoples of the north coming from all kinds of lands', as they are called in the Karnak inscription, did not pass through Ugarit and north Syria before uniting forces with the enemies of Mineptah in the delta, where they were to be killed, repulsed or captured by the Egyptian army about 1230 or 1220 B.C. This conclusion is not really inconsistent with what the inscriptions say about the results of the Egyptian victory at Perir, particularly the celebrated 'Israel' stele of the funerary temple of Mineptah at Thebes. Some historians have regarded this last inscription as merely a conventional recording of the peoples conquered by the

⁶ Compare for the last J. von Beckerath, 'Tanis und Theban', in *Aegyptische Forschungen*, Heft 16, 1951, 56.

⁷ See Hamit Z. Kosay, *Alaca Höyük Hafriyatı*, Ankara, 1938, 106; *Les Fouilles d'Alaca Höyük*, Ankara, 1951, plates 183 (1), 203 (middle); and also Schaeffer, *Stratigraphie Comparée*, pp. 289 ff.

⁸ Déchelette, *Manuel d'Archéologie* II, 200.

⁹ Compare for this Ebert, *Reallexikon*, IV, s.v. *Griffangelschwert*, *Griffzungenschwert*, and XI, s.v. *Schwert*. See too the criticism of E. Sprockhoff, *Die Germanischen Griffzungenschwörter*, 1931, and my remarks in *Enkomi-Alasia*, pp. 337 ff.

¹⁰ By reason of the proximity of the Cypriot copper-mines, the bronze industry and the manufacture of weapons was specially developed at Ugarit, see *Ugaritica* III.

¹¹ E. Drioton and J. Vandier, *L'Égypte*, 3rd ed., 1952, 430: J. von Beckerath, op. cit., 66.

¹² For the Karnak stele and the complementary inscriptions see Drioton and Vandier, op. cit., 430; von Beckerath, op. cit., 66: and more especially J. H. Breasted, *Ancient Records of Egypt*, III, 238 ff.

¹³ W. Wolf, *Die Bewaffnung des Altägyptischen Heeres*, Leipzig, 1926, plate xv (e) and Ebert, *Reallexikon*, XI, plate 144 (e).

PLATE I



SWORD OF MINEPTAH FROM UGARIT (RAS SHAMRA)

(see p. 226)

PLATE II



AIR-PHOTOGRAPH OF THE DEADMAN'S BURIAL, EYNSHAM, OXON., FROM THE WEST
showing also ring-ditch crop-marks to the north and north-east, and the gravel pit on Foxley Farm, in which the Beaker Culture cemetery was found
Ph. The late Major G. W. G. Allen, M.C.

Pharaohs which was used on the occasion of the victory of Mineptah.¹⁴ Others think that the passage refers to a campaign of the same Pharaoh in Palestine and Syria in the third year of his reign, and so before the battle of Perir.¹⁵ The Ugarit sword with Mineptah's name on it confirms the fame enjoyed by that king extending as far as north Syria, and might support the theory of a victorious campaign in Palestine and Syria at the beginning of his reign. If the Peoples of the Sea had not yet attacked Ugarit in the time of Mineptah, as our sword seems to indicate, it would be quite natural to suppose that they preferred to by-pass that stronghold¹⁶ and make their landing at some point further south in Canaan or Palestine. Their first acquisitions there might have been lost as a result of their defeat at Perir or during a specific campaign undertaken by Mineptah before confronting the Libyans, as Breasted, Drioton and Vandier think. In either case the Pharaoh would seem to have been justified in claiming to have pacified these lands and eliminated the subversive elements that had infiltrated into them.

This Mineptah sword from Ras Shamra provides us also with a well dated prototype for the weapons of the European Bronze Age, and for their chronology; and it is even more informative than the often cited sword with the cartouche of Sethi II (1210-5 or 1191-86) found in the Delta.¹⁷ This last, rather badly preserved weapon has been regarded as belonging to a type with a flanged or riveted hilt,¹⁸ (*Griffzungenschwert*),¹⁹ which includes also the sword from Enkomi in Cyprus.²⁰ The Mineptah sword, on the other hand, belongs to a slightly older type with a long narrow tang. In France, Déchelette²¹ put the oldest examples of this last type (*à lame effilée*) in his Bronze Age III (1600-1300 B.C.), putting those with flat hilts (*à soie plate*) like the Sethi II sword, in his Period IV.

The Ras Shamra sword confirms the typological development already proposed for swords with a narrow tang, according to which they are succeeded by the type with flanged and riveted hilts, designed to give a hold to the pommel. A comparison, on the other hand, with the swords in the hoard of seventy-four bronze tools and weapons found in the great priest's house at Ugarit²² shows that European prehistorians were right in thinking that the thrusting sword, with a rather narrow and slender blade, was earlier than the broad-bladed cutting sword, and that during the transition from the first to the second type there was developed a hybrid form²³, the cut-and-thrust sword of the kind to which belongs the bronze sword of Mineptah found at Ras Shamra.

¹⁴ See for the last expression of this view J. von Beckerath, *op. cit.*, 258.

¹⁵ Notably J. Breasted, *op. cit.*, p. 258. The same opinion is guardedly expressed by Drioton and Vandier, *loc. cit.*, p. 431.

¹⁶ See my provisional report of the Ugarit fortifications in *Syria*, 1939, pp. 289 ff.

¹⁷ W. Wolf, *op. cit.*, p. 73, plate 15: Ebert, *Reallexikon*, XI, s.v. *Schwert*, p. 439, plate 144.

¹⁸ Déchelette, *op. cit.*, series C, type 1, p. 208.

¹⁹ J. Naue, *Vorrömische Schwerter*, type II: E. Sprockhoff, *op. cit.*

²⁰ See *Enkomi-Alasia*, p. 337 ff.; *Stratigraphie Comparée* I, 418, fig. 222.

²¹ *Op. cit.* II, 106, 200 ff.

²² To be published shortly in *Ugaritica* III.

²³ Déchelette, *op. cit.*, p. 202.

Notes and News

ARABIC INSCRIPTIONS IN SOUTHERN ETHIOPIA

1. THE HISTORICAL SETTING. From at least the 9th century A.D. much of southern Ethiopia as far west as the upper Omo river (and probably even as far as the Didessa) was under Moslem control of varying degrees of effectiveness. The first Moslem state recorded was that of Shawā, roughly the modern Shoa, north of the Hawash river, which was founded according to tradition in A.D. 896 and was absorbed in A.D. 1280-5 by Ifat.¹ This state stretched from Zequala in Shoa to Zayla on the coast, and was a member of the so-called 'Empire of Zayla', known to Arabs as Barr Sa'd ad-Din from the ruler of that name who was killed in A.D. 1402. To the south of Ifat were Dawāro and Bāli. Dawāro was a long, narrow strip reaching down to the Webi Shabelle; in it lay Harar, Hobat, and Dakar, once capital of the 'Empire', the site of which I think is the modern village of Chenāhasan near Jiggiga. Bāli lay between Dawāro and the Lakes. West of the Lakes was Hadya, which certainly included the territories of Guragē, Hadya (Gudela), and Kambättā, and probably included the area where in the 16th century A.D. were established the Galla monarchies of the Gibē (Limmu, Jimma, Gera, Guma, and Gomma). Between Bāli and Hadya were two small states called Waj and Faṭagār. These states, with the exception of Waj and Faṭagār, were described by the Arab historians Al 'Umari (A.D. 1345) and Maqrizi (A.D. 1435). From their accounts, and from the evidence of the Ethiopian chronicles it is quite clear that up to about A.D. 1540 they were regarded as part of the kingdom of Ethiopia, but governed by semi-independent Moslem rulers who were nominally at least tributary to the king of Ethiopia, but revolted from time to time, and were in consequence visited by punitive expeditions from Ethiopia. Waj and Faṭagār fade out of the picture before the others, though their names survived. Waj disappeared about the end of the 15th century A.D., when it probably became part of Hadya, and Faṭagār by 1540 when it became part of Bāli.² The larger states revolted successfully against Ethiopia during the war with Grañ, the Moslem conqueror of Ethiopia and by A.D. 1540 were independent, the most prominent of them, Ifat, surviving as 'Adal' till 1517, and as 'Harar' till 1887, with capitals successively at Awssā, Dakar, and Harar. This very brief summary of a complicated situation explains how there came to be an area of strong Moslem influence in southern Ethiopia. This area was approached from Zayla by caravan routes through Harar. One was recorded by Salt in the early 19th century as going by Jaldēssā and Harar along the Harar uplands past Chalanqo to Ankobar in Shoa; another led to Hadya in the west by way of Lake Zeway. It is along these routes that the Arabic inscriptions which are the subject of this note were discovered.

2. TOPOGRAPHY. Eighteen inscriptions in Arabic were found between the years 1922 and 1926 by the French missionary Father Azais and his colleague Roger Chambard.³ They occur mostly in Bali, with four to the SE of Lake Zeway. All seem to have been in or near recognizable Moslem cemeteries. I say 'seem' because the account is not always as clear as it might be. At several places Azais and Chambard made some excavations in a very unskilled and ineffective manner, which failed to produce any evidence

¹ For this see Cerulli, 'Il Sultanato dello Scioa nel secola XIII', *Rassegna di Studi Etiopici*, 1, 1941, 5-42.

² Though shown as kingdoms on Almeida's map (cir. 1646), they are merely 'survivals', just as Almeida shows Damot and Gafat south of the Abay, though they had ceased to be there for nearly a hundred years when he wrote.

³ *Cinq Années de Recherches Archéologiques en Éthiopie*. Paris, 1931.

NOTES AND NEWS

whatsoever as to the date or ethnological affinities of the persons commemorated.⁴ It is only because two of the stones bear dates that it is possible to refer most of them to the 13th century A.D. Among them are two fine Kufic inscriptions which Dr D. S. Rice dates to about A.D. 1000.⁵ Three inscriptions were found at Chalanqo, the site nearest to Harar, and one of the stations on Salt's caravan route. Two occur at Batē and seven at Lafto, both west of Chalanqo and off Salt's caravan route. Two more occur at Heyssa, sw of Lafto, and still further from the caravan route. These are all in Bāli. The remaining four come from Munēssā, about five miles E of Lake Langano, and some 200 miles sw of the Chalanqo-Heyssa group. This site seems to have been close to the border of Waj and Bāli.

TABLE OF INSCRIPTIONS

No.	Site	Details	A. & C. Plate	Littmann
1	Heyssa	<i>Lam-alif</i>	XXXI, 4	p. 246, no. 13
2	Heyssa	<i>Lam-alif</i>	XXXI, 4	246 14
3	Lafto	Name and date	XXV, 2 rt.	240 4
4	Lafto	Name and date	XXV bis. 1	241 1
5	Lafto	—	XXV bis. 2	242 6
6	Lafto	Name	XXVI, 2, no. 3	244 9
7	Lafto	—	XXVI, 2, no. 2	243 8
8	Lafto	Illegible	XXVI, 2, no. 1	{ not seen by Littmann
9	Lafto	Illegible	XXVI, 2, no. 4	
10	Chalanqo	Koranic quotation	XXV, 1	238 2
11	Chalanqo	Koranic quotation	XXV, 2	239 3
12	Chalanqo	Koranic quotation	XXV, 3	239 5
13	Munēssā	Untranslatable	LXIII, 1	{ not seen by Littmann
14	Munēssā	Untranslatable	LXIII, 4	
15	Munēssā	—	LXIII, 3	
16	Munēssā	Name	LXIII, 2	
17	Batē	Kufic	XXIX, 2	245 11
18	Batē	Kufic; name	XXIX, 1	245 12

3. EPIGRAPHY. The text of the inscriptions was published by Paul Ravaisse in Appendix I to Azaïs and Chambard's book (pp. 283-309), and they were further studied by Eno Littmann in 1924 (with the exception of two from Lafto and the four from Munēssā which he did not see).⁶ Epigraphically, the inscriptions fall into two groups. The first, comprising all but two, consists of crude inscriptions in a local variety of semi-cursive script, as Dr Rice describes it, on deeply cut base-lines. Two of the inscriptions (nos. 1 and 2, Heyssa) carry geometrical figures which Ravaisse calls *lam-alif* signs symbolizing the formula 'there is no God but God'; the name Muhammad also appears written in reverse (in what the Germans call 'Spiegelschrift'), perhaps, as Dr Rice

⁴ As an example of their work I quote from p. 204. After reaching a depth of 1 m. 50 in a grave at Munessa they found 'a fine complete skull. This was enough for us, and we stopped work without finding any grave goods which could indicate any period or civilization whatsoever'.

⁵ In dealing with the epigraphy I am much indebted to my colleagues Dr R. B. Serjeant and Dr Rice, with whom I have discussed the problems. To Dr Rice especially am I grateful for the careful examination he made of Azaïs' photographs, and for his expert comments on the inscriptions.

⁶ 'Arabische Inschriften aus Abessinien', *Zeitschrift f. Semitistik u. verwandte Gebiete*, 1924. III, 236-46.

ANTIQUITY

suggests, copied from a banner. Four⁷ (nos. 3, 4, 6, Lafto and 16, Munēssā) have the names of the people commemorated. Two of these also bear dates A.H.666=A.D. 1267-8, and A.H. 662=A.D. 1263. Three have quotations from the Koran (nos. 10, 11, 12 at Chalanqo). Three have neither names nor dates, and four are illegible or untranslatable. The two dated examples are of course important because they enable the whole group to be dated to the 13th century A.D. The second group consists of two inscriptions in



Kufic characters, nos. 17 and 18, both at Batē, one bearing a name. On these, Dr Rice has kindly given me the following note: 'Both are undated but can be placed round the year A.D. 1000. Cf. for their palaeographic characteristics, the *alif* and *lām* with small triangular heads, the *nūn* and *rā* practically identical and curving below the base-line. (Cf. the inscriptions in G. Wiet, *Catalogue du Musée Arabe du Caire, Stèles funéraires*, vol. VI, Cairo, 1939, pls. v, no. 2067 dated 401/1011, and VI, no. 2089 dated 407/1016).

⁷ Four, that is, if Ravaisse's interpretation of no. 3 is correct. He reads *Shuhra*, as a feminine personal name, but Dr Rice prefers to read *shahr*, 'month'.

They are clearly the work of a foreign craftsman who has nothing in common with the scribblers of the first group. The strictly perpendicular lapidary style, especially of the stela lying on the ground (Azaïs et Chambard, pl. xxix, 2) could also be Persian (Cf. the inscriptions from Persepolis reproduced in E. Schmidt, *Persepolis*, Chicago, 1954). The date suggested by Dr Rice is of course quite possible, since we know that there were Moslem settlements in southern Ethiopia before that time—the state of Shoa, for instance, was then in its prime. It is clear that by the 13th century A.D. there was a fairly extensive Moslem population in Bāli and Ifat. In the reign of ‘Amda Syon (1312–42), only about 65 years after the dated inscriptions, there occur in the long catalogue⁸ of places which sent troops to the army of Sabr ad din king of Zayla the ‘kingdoms’ of Hobat and Harlā under a king (*negus*) and the districts of Upper and Lower Hasyā under a governor (*makuannan*). Hobat is the place called Hubeta on the GSGS maps (1 million and $\frac{1}{2}$ million) about 20 miles NW of Harar. Hasyā may represent the modern Heyssa (I am not sure of the proper spelling of this name); and Harlā suggests the giant Arla or Harla of local tradition to whom the Galla attribute many of the ancient sites in the Harar region. It is not clear, however, what type of people these were. The personal names in the inscriptions do not help, for they are all Arabic, and it does not follow that they were borne by Arabs. The fact is that we do not know what the population of Bāli consisted of in the early Middle Ages. To-day the population of northern Bāli is mainly Galla, but we have no evidence that they arrived there before the 16th century A.D., and in the 13th century the inhabitants may have been of Hamitic stock related to the Sidama group now living further west (though this is uncertain), with a Somali element, and perhaps settlements of Ethiopian (Amhara) military colonists here and there.

G. W. B. HUNTINGFORD.

DEADMAN'S BURIAL, EYNESHAM, OXON.

The great crop marks five miles west of Oxford and one mile west of the Thames, south of the Eynsham–Stanton Harcourt road (B 4449), opposite the entrance to Foxley Farm, although probably often noticed, have escaped comment until now.¹ They consist, in essentials, of an arc to which the road is roughly tangential, with another symmetrical but lesser arc. These together form a partial oval enclosure of great size.

It was photographed from the air by Major Allen in 1933,² by myself in 1953, and by Dr St. Joseph in the same year. FIG. 1 has been compiled from the significant marks which appear on the three series of photographs.

An obviously intentional gap in the enclosure lies to the south-west; another possibly intentional gap to the north-east is less clear. The large interruption to the south-east, however, is likely to have been caused by the enclosure ditch running off Summertown–Radley gravel on to Oxford clay—a rock which is unfavourable to crop marks.³ Elsewhere differences of crop in various fields make the marks erratic.

Certain ramifications of the main crop marks are caused by two roughly parallel but ill-defined lines of marks, merging in places, which cross the centre of the enclosure

⁸ BM. MS. Orient. 821: ‘Histoire des guerres d’ ‘Amda Syon’, ed. by J. Perruchon, *Journal Asiatique*, 1889, 8 ser. xiv, fol. 49.

¹ National Grid: SP/423078. O.S. 25-in.: sheet xxii, 11. The site is at about 220’ O.D.

² Allen negative 526. Ashmolean Museum.

³ The Summertown–Radley gravel and the flood plain alluvium of the Thames just fail to overlap, and the Oxford clay comes to the surface between them. The feather edge of the gravel shown on FIG. 1 is taken from O.S. geological map (Drift) sheet 236; it corresponds roughly with a line of springs south-east of the site.

from south-west to north-east (PLATE II). They correspond to the position on the ground of a ridge, which marks the pre-19th century Eynsham-Stanton Harcourt road.⁴ This road may originally have passed through the north-east and south-west gaps in the enclosure, but by 1782 it had encroached on the ditches.

An estate map of 1615 belonging to Corpus Christi College, Oxford, shows the area of the enclosure lying in the South Field of the three field system then in use.⁵ It does not show any ditch or bank. However, what appears to be a rectangular earthwork aligned north-north-east about 20 feet long by 12 feet broad and named 'Deadman's buriall' is marked in a position just inside and slightly north of the south-west entrance of the enclosure. Nothing significant shows on aerial photographs at this point, and it is possible that the earthwork was only an unusually large headland. However, the name is memorable, and I have transferred it here to the enclosure—without, of course, implying a sepulchral purpose!

In 1952 and 1953, three lines of resistivity readings were taken across the enclosure,⁶ beginning and ending clear of it, through A'-D', B'-E', C'-F'. Local areas of low resistance, confirming the presence of an enclosure ditch were found between A-A', B-B', E-E', and F-F'. A similar area symmetrical to B-B' was found at D-D' in the Oxford clay. An area at C1-C1', however, lies adjacent to the former road; another at C-C' lies in a similar position, but also appears to straddle one of a series of well-defined marks near the northern periphery of the enclosure.

The evidence then points to an oval enclosure ditch, complete except for a gap to the south-west and possibly another to the north-east. Nothing gives any indication as to where the bank stood.

Few possibilities of interpretation offer themselves. It could be suggested that here was an Iron Age enclosure of the type once existing at Cassington, Oxon.⁷; but major differences from the Cassington site rule the suggestion out. The position has no tactical value, the ditches are too wide, and not a scrap of Iron Age (or indeed any) pottery has been found despite frequent searches.

It is more likely, despite the uncertain position of the bank, that the site was a double-entrance henge monument of the type described by Atkinson.⁸ This being so, its position and size make it of more than common interest.

The smaller but similar nearby site of the Devil's Quoits, Stanton Harcourt, is undated,⁹ but Beaker sherds were found in a primary position in the ditches of the more complex site of the Big Rings, Dorchester, Oxon.¹⁰ The strip of Summertown-Radley gravel running through Cassington, Eynsham and Stanton Harcourt has been extraordinarily rich in Beaker Culture finds¹¹; and the cemetery at Foxley Farm¹² lay immediately adjacent to the Deadman's Burial (FIG. 1 and PLATE II). To the north

⁴ *Plan of the Manor of Ensham* . . . by Thomas Pride (1782). Oxfordshire County Records. The road is shown in its modern position on the Enclosure Map of 1800. Chambers, *Eynsham under the Monks* (Oxfordshire Record Society, XVIII, 1936), gives references to maps of Eynsham.

⁵ I quote by kind permission of the President and Scholars of Corpus Christi College, Oxford. The name 'Deadman's buriall' also appears in a terrier of 1697 belonging to the College.

⁶ Using the method evolved by Atkinson. *Field Archaeology* (1953), 31-8. Mr A. G. Fenwick, Dr W. Glasbergen and Miss E. Rutter kindly assisted at various times.

⁷ *Oxoniensia*, v, fig. 1, XVI, pl. 1; VII, 105-7, XVI, 79.

⁸ *Excavations at Dorchester* (1951), 81-107.

⁹ *Oxoniensia*, VIII-IX, 24-34.

¹⁰ *A.N.L.*, IV, No. 4, 58; *Oxoniensia*, XVII-XVIII, 216.

¹¹ *The Oxford Region* (ed. A. F. Martin and R. W. Steel, 1954), 79.

¹² *Oxoniensia*, III, 21-6.



FIG. 1. PLAN OF THE CROP-MARKS OF THE DEADMAN'S BURIAL, EYNSHAM, OXFORDSHIRE

and east (PLATE II) lie clusters of ring-ditch crop-marks¹³ which invite comparison with those which surrounded the Devil's Quoits.¹⁴ Representing as many of them as do the quarry ditches of ploughed-down barrows,¹⁵ they may be compared with the barrow groups surrounding Avebury and Stonehenge.

These are not idle comparisons, since whether the bank of the Deadman's Burial lay within its ditch or outside of it, only those of Avebury and Durrington Walls are likely to have exceeded it in extent.¹⁶

HUMPHREY CASE.

ABINGDON WARE

Excavators of British Neolithic sites may often have been puzzled by the crumbly texture of the sherds which they have found. Most of those which I found in 1954 in the outer ditch of the causewayed camp at Abingdon, Berks., were hardly more cohesive than the ditch's filling. Mr E. T. Leeds had remarked in 1927 that the shell-gritted ware from this site was 'exceedingly friable', and that once the surface had gone a sherd would crumble 'almost at the least touch'.¹ Such ware seems useless.

However, its exceeding friability when found is probably due to water having invaded its laminated structure at the breaks. When new it seems to have been sturdy and serviceable, since it was made into large thin-walled vessels.² It was decided to try and discover experimentally how serviceable it had been.

A wall sherd $\frac{5}{16}$ inch (8 mm.) thick was selected of the coarsest and softest shell-gritted ware; the slip on its outer surface was fairly intact, but there was none on the inner surface which was slightly pitted. It had been excavated in 1926, so that any water which it had absorbed had long since evaporated.

A disc $1\frac{1}{2}$ inches (3.8 cm.) in diameter was cut from the sherd. Its edges were sand-papered, when a densely laminated structure of overlapping fragments of shell could be seen. The disc was then placed in the mouth of a glass funnel, and its edges luted to the wall of the funnel with putty; and a continuous coating of putty was used to line the wall of the funnel from the joint upwards. A vessel of about 65 c.c. in capacity was thus formed with a water-tight wall, and a base of Abingdon ware. (Care was taken to avoid making the disc less porous by smearing putty on its surfaces or handling it with oily fingers).

A series of crude tests followed.

Test 1. The vessel was filled with cold water (56°F.; room temperature 60°F.) After 28 hours 45 minutes, when no water had seeped through the disc, the vessel was dismantled. The disc had absorbed water, but its surfaces were unimpaired and its structure negligibly softened.

After 51 hours 45 minutes tests were resumed and continued consecutively.

Test 2. The vessel was reassembled and filled with warm water (100°F.; room temperature 68°F.), which was changed every 5 minutes. After one hour when no water had seeped through the disc, the vessel was dismantled, when the disc was found to be in the same condition as after *Test 1*.

¹³ *Oxoniensia*, VI, 85-6, and pl. XI.

¹⁴ *Oxoniensia*, VIII-IX, fig. 8.

¹⁵ It is interesting to see the area north of the Eynsham-Stanton Harcourt road and of the Deadman's Burial, called 'Barrow Hill' in 1615, and 'Round Hill Furlong' in 1782.

¹⁶ Cf. Atkinson and others, *Excavations at Dorchester*, fig. 27. The site at Marden, Wilts. (loc. cit., 104), is left out of the question.

¹ *Ant. J.*, VII, 450.

² Loc. cit., 454.

Test 3. The vessel was reassembled, and filled with boiling water, which was changed every minute; it was poured in and out briskly, with no particular attempt at caution. After 7 minutes, a few particles of clay had been dislodged from the inner surface of the disc. After 29 minutes, 28 c.c. of water had seeped through the disc; and after 46 mins., 56 c.c. After one hour, when 81 c.c. had seeped through the disc, the vessel was dismantled. Apart from the insignificant damage suffered after 7 minutes, the disc was in the same condition as after *Test 1*.

Test 4. The disc was immersed in water boiling in an aluminium saucepan. After boiling for one hour, the disc was examined. The inner surface was slightly more pitted; the edge had flaked very slightly; and the shell grits exposed on the inner surface and the edge had been rendered rather soft and chalky. With these exceptions, the disc was in the same condition as after *Test 1*.

Test 5. A small quantity of water was collected on the inner surface of a sherd of medium quality shell-gritted ware from the 1926 excavations. The outer surface was then placed over a naked flame. The sherd broke in less than a minute.

The tests show that a vessel made from the worst quality Abingdon ware must have been quite serviceable—all the more so, since in primitive use its inner surface would probably have been greased in some way or other,³ which would have made it less porous.

It could have stored water for a reasonable period (*Test 1*) and probably long enough to make cider, beer, or mead. It could have been used for milking (*Test 2*), and for serving and handling very hot liquids or semi-liquids (*Tests 3 and 4*)—punch, porridge, or stew for instance.

Its uses in cooking would obviously have been limited. Baking in it should have been feasible. *Test 5* was hardly necessary to show that any heating over a naked flame was out of the question. A certain amount of boiling and simmering may have been possible—for, provided the vessel was sunk in a pit, or packed round with earth and turves, it should have been possible to boil water by dropping in hot stones. The liquor would have needed frequent topping up, for in *Test 4* the vessel lost about a quarter of its contents in the first twenty minutes, and thereafter seepage was roughly twice as fast. The attention may have been worth while; for the meal, if no tastier, would have been superficially cleaner than one made in a cooking pit!⁴

HUMPHREY CASE.

THE INSCRIPTION OF ST. GEORGE OF BAHA

We are indebted to Mr G. W. B. Huntingford for inviting attention to the 16th century Ethiopian monophysite church of St. George at Baha in the Kafa region of Ethiopia.¹ This church was founded in the reign of King Sarša Dengel (1563–1597), and the circumstances leading up to this event may be studied in C. Conti Rossini's edition of the Ethiopian *Historia Regis Sarša Dengel*.²

³ For instance by the method described by the late Sir Lindsay Scott in *History of Technology* (ed. Singer and others), I, (1954), 381.

⁴ Stew or fruit, for example could not have been wrapped like Professor O'Kelly's leg of mutton in the cooking trough. *J.R.S.A.I.*, LXXXIV, 122.

¹ *ANTIQUITY*, 115, September 1955, 162–4.

² *C.S.C.O.* 1907, 120 seqq. See also Sir E. A. Wallis Budge's *History of Ethiopia*, vol. II, 359–75; Luca dei Sabelli, *Storia di Abissinia*, vol. II, 236–8; *Guida dell'Africa Orientale Italiana*, 539.

The *tabot* ('altar-slab', 'Ark of the Covenant', the principal item of furniture of every Ethiopian church)³ of this church bears an Ethiopic inscription which was published by Cecchi in 1885, unfortunately in a very corrupt form. Huntingford (loc. cit.) reproduces Cecchi's 'copy'⁴ together with his own 'possible alternative version' of the text and a translation. Neither Cecchi's nor Huntingford's versions, however, can claim to represent a tolerable Ethiopic text, and the limited object of this note is simply to establish a text which (a) is reasonably faithful to Cecchi's copy; (b) conforms to the requirements of the Ethiopic language, and (c) produces a meaning that can be justified on the basis of the historical information available to us:

1. ዘ ታቦት : ዘ ገላድ : ደ ገ ገል :
2. ታቦተ : ቅዱስ : ጊዮርጊስ :
3. ወእ ገዝእ ትነ : ጣርያዎ :
4. ዘ ታቦት : ለዘአዎላክ : [ቢት]
5. ዘሀነስ : ሠርፀ [ደ ገ ገል] ገር[ጣድ : ገጉሥ]

1. This is the *tabot* which Dengel⁵ had covered⁶
2. The *tabot* of St. George
3. And of our Lady Mary
4. This *tabot* belongs to the House⁷ of God
5. Which has built Sarṣa Dengel⁸, His Majesty the King.

EDWARD ULLENDORFF
Department of Oriental Languages,
The University, St. Andrews

³ Cf. the present writer's forthcoming study on the 'Hebraic-Jewish elements in Abyssinian (monophysite) Christianity', especially the chapter on the Ark of the Covenant.

⁴ I have not got access to Cecchi's book at the time of writing and am therefore relying on Huntingford's reproduction.

⁵ Dengel (=Virgin) could here stand as the component element of either Sarṣa Dengel or Za-Dengel (or Za-Maryam), the baptismal name of Badantcho, the chief of the Enarya (see Conti Rossini, loc. cit.).

⁶ The covering of the *tabot* is the act of consecration, for it is the *tabot* and not the church building which is consecrated. Cecchi's *gulo* is an easily explicable 'corruption' of *gäläwwo* and need not necessarily be due to faulty copying. For *gäläwä* in connexion with the Ark of the Covenant, see Dillmann, *Lexicon Linguae Aethiopicae*, col. 1140.

⁷ 'House' is supplied at the end of the line. The construction shows the influence of Amharic syntax.

⁸ 'Dengel' is supplied, but the dots in Cecchi's copy (p. 164 in ANTIQUITY, 115) indicate an omission. The same applies to the end of the line.

Correspondence

29 September 1955

DEAR MR EDITOR,

May I venture a brief comment on your characterization (ANTIQUITY, June 1955, p. 76) of the recent article by Hjalmar Torp in *Acta Archaeologica* about the Vatican excavations as 'a scholarly analysis of a bad excavation'?

The article itself is unimportant. It makes one or two shrewd points. But its main conclusions are based on assumptions about the masonry of the individual tombs that nobody familiar with the site (which, is, after all, now readily accessible) could possibly have made. The result is a house of cards, amusingly and ingeniously constructed but not to be taken too seriously. Those who are interested will find some of the arguments discussed more fully by Professor Toynbee and myself on pp. 268-70 of our forthcoming book, *The Shrine of St. Peter and the Vatican Excavations* (Longmans, Green and Co., January 1956).

My purpose in writing, however, is not to dilate on what I believe to be a shortcoming of an ephemeral article, but to protest against the dismissal as 'bad' of one of the most scrupulously conducted and meticulously published excavations ever undertaken in Italy. One may deplore the conditions of absolute secrecy in which it was carried out, a secrecy which has inevitably engendered an atmosphere of scepticism and prejudice regarding the results. But it is only fair to the excavators to point out that this was a matter over which they had no control whatever. Their responsibility was limited to the day-to-day supervision of the actual digging and to the preparation of the published account; and here I think that any fair-minded person familiar with the site and with the conditions under which they worked will be bound to admit that they did a pretty good job in the face of great technical difficulties. There are, inevitably, features of their work that one can criticize. It might have been possible to extract more evidence of the stratigraphic relationship between the all-important early graves and the natural surface-line; I should have hated the job myself, having to dig upwards from below through a densely packed tangle of early graves and footings, but it is theoretically possible. Again, one may question the chronology ascribed to the same early graves. Here the report comes dangerously close to special pleading. The important thing, however, is that in this, as in almost every other case, the essential facts are noted and that they are presented clearly and honestly: the evidence for agreement and disagreement is all there. Apart from its outrageous price, it would be fairer to describe the report as a good and scrupulous account of a difficult piece of work well done.

Yours sincerely,

JAMES WARD-PERKINS.

[Excavations carried out in secret were bound to lead to different interpretations of facts. EDITOR].

Reviews

HANDBOOK OF THE GREEK COLLECTION. The Metropolitan Museum of Art. By GISELA M. A. RICHTER, 322 pages including 130 with collotype plates. Harvard University Press (London : Geoffrey Cumberlege), 1953. English price, £5.

The Handbook of such a rich and varied collection as that in New York, and from the hand of a scholar with such wide interests, as has Miss Richter, becomes more a History of Greek Art than a Museum Guide. This has been true of the preceding six editions of the Handbook (the first in 1917) and is only slightly less true of this in that the point has been reached at which one book can barely serve both purposes. The book comprises a description of the objects in the collection in narrative, not catalogue, form, each group being prefaced by a general introduction to the period or style discussed. In this way a continuous story of Greek art is told and illustrated by the collection itself (for those lucky enough to be able to visit it) and by over 800 photographs (for those not). But this is not a book to carry in the hand around the museum : for one thing it does not guide by rooms and cases but by period, and for another the price and format are beyond the means and requirements of the ordinary visitor. The scholar, who may still be a little dismayed at the price, is better served. Miss Richter is generous with dates and her sympathetic and accurate comments happily reflect the archaeologist rather than the 'art historian'. A useful appendix of footnotes records inventory numbers and original publications of all pieces mentioned, nearly all of which are also figured in the small but clear photographs, some of them for the first time (e.g. the Lion Painter fragment, pl. 26e). Slips of the pen like 'Eretria in Boeotia' (p. 157) are venial. It is interesting to note that two lacunae in our knowledge of early Greece which she mentions in her introduction to the Prehistoric Period (p. 7 f.) have been filled, by the recent decipherment of the Mycenaean script by Dr Ventris, and by the discovery during the war of palaeolithic remains near Haliartos in Boeotia : semper aliquid novi.

JOHN BOARDMAN.

THE GREEK HOUSE : its evolution and its relation to the houses of the other Balkan peoples. By GEORGE A. MEGAS. (*Series of Publications of the Ministry of Reconstruction*, No. 37). Athens, 1951. 134 pp., 75 textual figs., 18 plates of plans.

Students of the development of vernacular architecture and of the evolution of the house will welcome this very generously illustrated book which summarizes much of the work done in the past and published in little known languages. The author, the Professor of Folklore at the University of Athens, divides his work into three sections : the various forms of the Greek popular house, the house of the Slav peoples of the Balkans (i.e. Bulgarians and Yugoslavs) and the houses of the Albanians. The work is primarily of value for the presentation of material, and not all readers will agree with Professor Megas' view that most of the house types are to be traced to Greek inspiration.

The publication of such a work reflects greatly to the credit of the Greek Ministry of Reconstruction : in this country the Ministry of Town and Country Planning is busy pulling down such buildings.

IAN WHITAKER.

CONSILIIUM PRINCIPIS. By J. A. CROOK. Cambridge University Press, 1955. 27s. 6d. net.

The practice of the Roman Emperors of consulting their 'friends', the *amici principis* in matters of policy, particularly at times of crisis, is a familiar one to all students of the Roman historians. This consultative body, called for convenience the *consilium principis*, is clearly of major importance in the history of the government of the Empire and it has been treated by a number of eminent scholars, either in monographs or in

comprehensive works, such as the *Staatsrecht* of Mommsen and the encyclopedias. To assess the influence of the advisers of the Emperors, whether in formal committee or in informal consultation, upon legislation or judicial procedure is a matter of great intricacy, and views have differed widely, with the result that general references to the *consilium* have tended to be vague and confused. Mr Crook's aim in this book is to dispel such confusion and to provide for the first time a full account of the subject in English. To this end he has made an exhaustive study of the older literature together with some important new epigraphical texts and the papyrus documents known as the 'Acts of the Martyrs', and examines the whole question anew with admirable clarity as far as the inadequate sources permit. He draws a clear distinction between the functions of the senatorial advisory committee of Augustus and the part played by the more informal groups of *amici*. Particularly he challenges the view, which has been widely held, that Hadrian and Diocletian organized the *consilium* on formal lines that led directly to the *consistorium* of the 4th century. His treatise is an excellent piece of work which is likely to remain the standard treatment of the subject for many years.

Moreover, apart from the intricacies of legal and constitutional development, Mr Crook emphasizes the importance of the *consilium* as a group of powerful and experienced men who moulded imperial policy and influenced its continuity over long periods. In a prosopographical index the names and careers of three hundred and sixty persons are listed who are known to have been *amici principis*. Some are well known figures; of others little can be recorded except their names and official offices; yet their quality as distinguished soldiers and administrators is apparent, and it was natural that the emperors should lean upon their judgment and experience. To take a name almost at random, Sex. Iulius Frontinus, the great soldier, governor of Britain for some three years, three times consul, twice with Trajan for his colleague, was *amicus* successively of the Flavians and of Nerva and Trajan, and accompanied Domitian as *comes* against Civilis and in the Chatti war. Many entries reveal the hazards of those who shared the power and secrets of the court: M. Vestinus Atticus, *amicus* of Nero, was forced to commit suicide: Iunius Silanus, *amicus* of Claudius, was executed in A.D. 42. M. Licinius Crassus, *amicus* of Tiberius and Claudius, accompanied the latter to Britain with his son, but both were executed in A.D. 47, and so the melancholy tale is often repeated.

By focusing our attention upon those who played so important a part behind the scenes Mr Crook's book helps us to understand the day to day working of the machine of imperial government and how the whole complex organization managed to survive the repeated perils of arbitrary rule by the steadying influence of many devoted and experienced men. G.F.F.

NICCOLO PEROTTI'S VERSION OF THE ENCHIRIDION OF EPICTETUS.

By REVILO P. OLIVER. *University of Illinois Press*, 1954. \$4.00.

Niccolo Perotti was a notable figure among the Humanists of the Italian Renaissance in the 15th century and has his place in the history of scholarship. He was the author of the first modern treatise on Latin prosody and of the first systematic Latin Grammar, which, after its first reproduction at Rome in 1473 by what Perotti in one of his letters calls the '*novum scribendi genus e Germania nuper ad nos delatum*' maintained its place as a standard work for a century, and was described by Erasmus as the most complete manual extant in his day.

Other works of erudition contributed to Perotti's fame among his contemporaries but his Latin version of the Enchiridion, presented to Pope Nicholas v within a year or two of its completion in 1450, remained so little known that it was Politian's version,

given to Lorenzo il Magnifico twenty-five years later, with the implication that it was the first Latin rendering, which became the standard Latin version for many years.

It is only now, after the lapse of five centuries, that Perotti's translation appears in a printed copy, based upon a most careful collation of twelve of the thirteen MSS. in which it is found; the thirteenth is a late copy of no independent value. Perotti himself, one suspects, might be surprised to find his version endowed with a full *apparatus criticus* and edited with a wealth of scholarly detail reserved in his own age for the writers of antiquity.

Mr Oliver has done his work with an admirable thoroughness and shows himself a master of the very extensive literature comprised in the writings of the Italian Humanists and their historians. He calls attention in a second apparatus to all the passages in which Perotti's version differs from the accepted Greek text in both the Introduction of Simplicius, which the volume also includes, and in the Enchiridion, and examines significant details which may suggest the relationship of the manuscript which Perotti used to those upon which the Greek text is based. Some affiliations do emerge but no very definite conclusions can be drawn.

Mr Oliver's introduction contains much interesting material. He points out that the admirable manual of morality of the gentle Stoic teacher can have had little appeal to the swashbuckling age of his translator 'when a thousand lances or a good Latin style might make any man a power in Italy', and writes in a footnote: 'It is significant that Politian, seeking to commend his translation of the Enchiridion to his contemporaries, felt it necessary in his prefatory letter to defend Epictetus from the imputation that he wrote for the humble and unfortunate. The precepts of meekness of the Enchiridion did not deter its translators or their contemporaries from the most extravagant invective against one another in their practice of what Mr Oliver, with transatlantic fondness for neologisms calls 'the art of loedorography'. He quotes at length an amusing but outrageous diatribe by Perotti against a papal secretary, George of Trebizond, where the original cause of the quarrel appears to have been a difference of opinion on the writings of Plato.

Mr Oliver reveals in his commentary the command of a fluent Latin style worthy of his theme. He offers his work as a contribution to the study of the Renaissance, which he designates the *Hauptproblem* of modern history. It must be admitted that the results of so much labour are somewhat meagre, but one may allow his claim that such detailed studies establish a firm basis for the assessment of the part played by the Italian Humanists in the cultural changes in which they participated and which they seemed to initiate.

G.F.F.

CARCHEMISH. Report on the Excavations at Jerablus on behalf of the British Museum. Part III: *The Excavations in the Inner Town*, by SIR LEONARD WOOLLEY, and the *Hittite Inscriptions*, by R. D. BARNETT. Pp. 157-290, figs. 62-96, pls. 29-71, AA, AB, AC, A 19-33, B 33-70. Published by the Trustees of the British Museum, London 1952. £6.

The report on the results of the British excavations at Jerablus, the ancient Carchemish, on the middle Euphrates, is now complete. The first two parts with an introduction by D. G. Hogarth and a description of the fortifications of the town by C. L. Woolley appeared in 1914 and 1921. Thus a lifetime has passed since their publication. It was for long hoped that it might be possible to continue the excavations done during 1911-14 and 1919-20, but this hope had to be abandoned: Jerablus is a strategically important point on the Turkish frontier, and no non-Turkish expedition would get permission to excavate there.

In chapters VIII–XV of this third volume Sir Leonard Woolley deals successively with the Lower Palace area, the so-called ‘Hilāni’, the Herald’s Wall, the King’s Gate, the Acropolis Mound, the Pottery Sequence, the Sculptures and the Small Objects. Of the Lower Palace itself, which was built in terraces on the slope of the hill, not much has been preserved. But a monumental flight of stairs leading to the Upper Palace and a Temple of the Storm-god adjoining the latter laterally were uncovered. The front of this temple, which had to be by-passed to reach the stairs, and the sides of the stairs were richly decorated with reliefs showing gods and war scenes (chariots and warriors with prisoners). Inside the temple, in front of the cella an enormous block of basalt was uncovered with two bulls cut into it. This block served probably as base for a statue of the Storm-god or for a big water basin.

South of the temple and the staircase, immediately adjoining the terrace, remnants of a nearly square building (18.50 by 19 m.) were uncovered. A doorway decorated with columns was reached across a ramp and beyond that a courtyard and a single enclosed room. There a few steps of a flight of stairs were preserved leading to an upper room. The whole arrangement obviously represents a Hilāni, a building of that special type which had been created in Syria and which was later repeatedly copied by the Assyrian kings.¹ One of the most interesting finds was made at the outer wall of the Hilāni—the basalt statue of a prince, without a head and with his hands on his knees. A cuneiform inscription on the front of his dress and on his back was carefully obliterated. Nevertheless there can be little doubt that the statue represents the Assyrian king Shalmanesar III (858–824 B.C.) which had not been realised up to now. A precise parallel with the inscription preserved was discovered by A. H. Layard in the ruins of the ancient town of Assur and is now in the British Museum.² The arrangement of the hands is conclusive: it is typical of numerous Egyptian statues. In Western Asia, however, this posture occurs only, as far as monuments of the cuneiform-script civilization are concerned, in these two seated statues from Assur and Carchemish.³ Why Shalmanesar, and he only, had had himself represented in such an un-Assyrian posture, and whether we have here perhaps Egyptian influence by way of Syria, is difficult to say. The Assyrian king, moreover, did not himself conquer Carchemish, though he did conquer the places in the immediate neighbourhood and received tribute from Sangara of Carchemish. Probably the statue was carried away as war booty from a neighbouring town to Carchemish in later times.

South-west of the temple and the palace stairs was an enormous gate, the ‘King’s Gate’ of the excavators. The gate and above all the adjoining large courtyard with diverging walls were richly adorned with sculptured reliefs representing the king and his family, dignitaries, priestesses, musicians, ministrants, warriors and so on. Two lion-shaped bases carried one standing and one seated statue of a god. Illustrations of most of these sculptures had already been published in the second (1921) volume. Woolley rightly emphasizes that the reliefs show a remarkable difference in their treatment. Probably they are not contemporary. Woolley distinguishes in particular four periods covering the period 1000–700 B.C.

¹ cf. H. Weidhaas, *Der bit hilani: Zeitschrift für Assyriologie* 45, 1939, pp. 108 ff.; B. Meissner und D. Opitz, *Studien zum Bit Hilāni im Nordpalast Assurbanaplīs zu Ninive*, Berlin, 1940.

² Illustrated in B. Meissner, *Könige Babyloniens und Assyriens*, Pl. 10 opposite p. 144.

³ Apart from a small Old Babylonian statuette, cf. Weidner, *Archiv für Orientforschung* 4, 1927, p. 133 ff.

The eastern wall of the courtyard behind the King's Gate turned at right angles and continued as the co-called 'Herald's Wall'. Here also a row of orthostats was uncovered, only partly in a good state of preservation (all of them already illustrated in volume I of the publication, 1914). They show mostly mythological scenes, and it is noteworthy that—in marked contrast with the reliefs in the courtyard of the King's Gate—each slab shows a whole scene and has no relation to the adjoining slabs. Exactly the same thing was observed in Bossert's excavations in Karatepe. Woolley thinks that these reliefs belong to an earlier period than those in the area of the King's Gate. Thus the 'Herald's Wall' might perhaps be the remnant of an earlier building which had been incorporated into the later construction of the palace. It is difficult to obtain definite results here, as there have been no excavations inside the wall.

The Acropolis Mound of Carchemish is divided into two peaks by a depression. On the south-easterly peak a rich stratification was observed, from the chalcolithic period to modern Arab times. During the chalcolithic period there had been an open settlement which grew up through the centuries with ever new buildings to a height of 29 m. above the level of the Euphrates. Later a strong fortress was built on the top; and then the whole site was levelled and surrounded with a wall, constructed first of sundried brick and afterwards, in the Late Hittite period, of stone. Probably this wall enclosed the Upper Palace reached by the monumental staircase mentioned above. During the time of Roman rule (2nd–3rd centuries), the top of the south-easterly peak was crowned by a temple, probably dedicated to the sun-god, the enormous foundations of which are still preserved.

On the top of the north-westerly peak also remains of Arabic, Hellenistic and Byzantine times were discovered. Under these layers a building with thick walls of dry brick was found, and in it, in a hearth, a brick with the inscription 'Palace of Šarrukênu, King of Nations, King of Assyria'. It was thought at first that a fortress had been built by Sargon II, but this brick remained a single specimen and had certainly been brought there from another place. Woolley is now of the well-founded opinion that the north-westerly peak carried the temple of the goddess Kubaba who was regarded as the 'great queen of Carchemish'. Only a small part of the sanctuary could be excavated. Of the finds a lion column-base and an altar with a hieroglyphic inscription are noteworthy. Underneath the temple burials only were discovered.

There are a great many Hittite hieroglyphic inscriptions from Carchemish. When first discovered they were as a book with seven seals. Today their decipherment is, thanks to the efforts of Bossert, Meriggi, Forrer and Gelb, well advanced and likely to produce reliable results; and the bilingual inscriptions of Karatepe have also yielded important new information. Thus R. D. Barnett has been able, in the 16th chapter of this publication, to show important progress in the interpretation of the inscriptions. They start towards the end of the 2nd millennium B.C. and reach to approximately 750 B.C. Barnett has succeeded in compiling a list of the kings of Carchemish which begins at about 1350 B.C. and goes on to about 715 B.C. There are, still, of course considerable gaps (some supplementary evidence and corrections have been provided by the new finds at Ugarit), but at least there is now a solid foundation on which to build. This is of great importance for the dating of the monuments which for several decades had been highly controversial.

In the 14th chapter Woolley has taken a lot of trouble to fix the date of the sculptures on the evidence of the inscriptions and the style. His conclusions are proof of his profound knowledge of the material, but have not produced definite results in every respect. Thus the discussion has started again. It will be sufficient to refer to the

statements of Machteld J. Mellink⁴ and of H. G. Güterbock.⁵ Perhaps concerted effort will succeed in working out the principles of the new and late Hittite art and in attaining fixed dates for the individual phases of development.

It is certainly, from the scientific point of view, a very saddening fact that the excavations at Carchemish could not be continued and brought to a final conclusion. The Turkish authorities however were well advised in having the monuments transferred from the site to the Museum at Ankara, and thus preserved from destruction. Only a few important pieces have vanished or are to be regarded as definitely lost. On the site itself only one great relief was left⁶.

Of the excavators who worked at Carchemish only Sir Leonard Woolley is still alive. We have to be sincerely grateful to him for completing the great work on the results of the excavations. This splendid and informative volume is an additional and fine contribution to the many he has already made to the advancement of knowledge of the Ancient East.

ERNST WEIDNER, Graz University.

CATALOGUE OF GREEK SCULPTURES IN THE METROPOLITAN MUSEUM OF ART, NEW YORK. By GISELA M. A. RICHTER, XVIII + 123 pages, 11 figures in text, 164 plates. Oxford, Clarendon Press, 1954. £7 7s.

This volume comprises the Greek sculptures in stone, whether originals or Roman copies, in the Metropolitan Museum; Roman portraits and marble sarcophagi, as well as the Cypriot collection, are excluded since they have been treated elsewhere. The Greek sculpture collection, numbering 245 pieces of which all save two have been acquired in the present century, is select and well tended, and has not yet grown too large to be adequately published in a single-volume catalogue. Miss Richter gives an objective description of each piece, with a full bibliography. Her comment is sound and well-informed, and she makes the most of her material; the one weakness seems to be occasional inaccuracy in the rendering of inscriptions and use of ancient literary sources. A novelty, in line with the latest epigraphical research, is the under-current of scepticism about the identification of the different Greek marbles. The photographs, of which many are new, are almost uniformly good and satisfactorily reproduced in half tone. With the sculptures in the round Miss Richter has concentrated, perhaps too rigidly, on the four cardinal viewpoints; a three-quarter shot might sometimes have been more illuminating than the second profile view.

The catalogue begins with the magnificent Early Attic Kouros, which Miss Richter dates in the closing years of the 7th century B.C. and which could even be appreciably earlier to judge by comparison with painting. Archaic grave stelae are well represented and include the now nearly complete 14 feet high grave monument no. 15. The classical gravestones are also an important collection, among them the Brocklesby stele and some good sculptured vases. The value of the catalogue is enhanced by a brief up-to-date introduction to each of these classes. Quite a number of the well-known statue types are represented by Roman copies; some, like the crouching Aphrodite of Doedalsas and the 'Narcissus' and 'Venus Genetrix' types, are headless or mutilated, but the 'Protesilaus', the Lansdowne Amazon and the restored Diadumenus are outstanding examples. The description of each of these copies is accompanied by a clear, concise

⁴ *American Journal of Archaeology* 58, 1954, pp. 247 ff.

⁵ *Journal of Near Eastern Studies* 13, 1954, pp. 102 ff.

⁶ cf. H. G. Güterbock, l.c., pp. 102.

discussion of the type. With her profound knowledge and her ability to outline conflicting views in simple terms Miss Richter is particularly at home here, and both students and teachers will regret that the limitations of the New York Collection did not permit her to discuss a much larger number of ancient statue types in the present volume.

J. M. COOK.

DER SCHATZFUND VON GROSS BODUNGEN. By WILHELM GRÜNHAGEN. (*Römisch-Germanische Forschungen* vol. 21). Berlin : Walter de Gruyter & Co., 1954. pp. 81, 16 plates. Price not stated, but ascertained to be DM. 28.

This is a useful study of a casual find of late Roman gold coins, fragments of silver plate and of other objects, which came to light in a potato field near Gross Bodungen in Thuringia in 1936 and was in due course acquired by the Halle Museum ; the author's study of it began as a doctoral dissertation and has now been developed into a worthy addition to the series of archaeological monographs in which it has found a place. The text devotes separate chapters to the discovery (with a reasoned inventory of the objects included in it), the fragments of decorated silverware (with two excursus on allied material), Roman silver scrap in general (with a welcome list of comparable finds, including those at Coleraine and Limerick in Ireland and Traprain Law in Scotland), and the question who buried the hoard and when. The 21 coins start with one of Magnentius which had been used as a pendant, and end with five of Arcadius, seven of Honorius and five of Constantine III ; the silver plate includes portions of a circular dish showing an emperor seated and flanked by soldiers, convincingly identified as the product of some imperial *largitio* (with a strong probability that it was issued by Magnus Maximus on the occasion of his *quinquennalia* early in 388), and fragments of a dish with a frieze of fishes and fisherman and in the centre a representation of the legend of Hylas. But the most exciting thing about the book is its demonstration that silver scrap was in widespread use within the Roman empire in the 4th century, for making payments of substantial sums ; hoards like that from Traprain Law need no longer be attributed to vandalism of looters making a rough and ready apportionment of their booty : they might even represent payment in convenient form, made to Votadinian *foederati*.

ERIC BIRLEY.

BEDE'S ECCLESIASTICAL HISTORY OF THE ENGLISH NATION. *With an Introduction by* DOM DAVID KNOWLES. *Everyman's Library*, No. 479. 1954. J. M. Dent and Sons, London. 6s.

The present volume contains an unaltered re-issue of the translation first published in this series in 1910. This was a composite work, being that of John Stevenson (1870), revised by L. C. Jane (1903). As in the earlier *Everyman* volume, the present text also includes *The Life and Miracles of St. Cuthbert* and *The Lives of the Abbots of Wearmouth and Jarrow*.

That the present text is a re-issue, and not a revision, should not be a matter for regret, for the excellence of this translation admits of no doubt. It is an obvious truth that any translation, however good, must always have the nature of a compromise. It must be accurate and faithful to both the letter and the spirit of the original, but anything savouring of slavish imitation or conscious archaism must be avoided ; it must also possess the virtue of clarity and be able to be read on its own merits, and endeavour as far as possible to sound like an original work in the translator's own language. On all these counts the translation here offered can be said to have been eminently successful, and the present reprint is therefore fully justified. In Bede's Latin, 'which he wrote

with great purity and simplicity' (thus the *Introduction* to the present volume), there naturally occur many long periods which must be rearranged or broken up in an English version; but where this is done (and the *Preface* alone affords several instances of such a procedure) the translation is always clear and readable, never obscure or mannered. A word or phrase of the original Latin is sometimes omitted, but only where its loss is of little or no importance. Thus in Book III, chapter 17, it is not really necessary for the modern reader to know that the church containing the post on which Aidan was leaning when he died was burnt down as the result of someone's carelessness (*per culpam incuriae*, omitted in the translation); the point is that the post was not burnt, although the rest of the church was destroyed.

The *Introduction* to the earlier volume, by Vida D. Scudder, was mainly concerned with the literary, especially poetic, elements in Bede's work, and suffered somewhat from a too florid and rhetorical style. The *Introduction* to the present volume occupies only half the length of its predecessor, and has been entirely re-written; it is concerned not so much with the literary quality of the *Ecclesiastical History* as with the author himself and his times, facts which are naturally of greater value to the scholar and historian, and indispensable to the reader who wishes to arrive at a true and just estimate of Bede's achievement in what is rightly held to be his greatest work. It is, in short, written from the point of view of the historian and not of the *littérateur*.

Other features of this volume are a short but up-to-date bibliography and—a great improvement—an index of proper names, which was lacking in the earlier issue. The notes to the text are unchanged in the essentials, though there are some additions. A few misprints in the original volume have been corrected.

K. R. BROOKS, Southampton University.

MEDIEVAL NUBIA. By P. L. SHINNIE. Museum Pamphlet No. 2, published by the Sudan Antiquities Service for 5 piastres or 1s. Khartoum, 1954.

This is an excellent summary, in eighteen pages, of an obscure and little known phase of history. Though disclaiming any originality and based chiefly on the publications of Monneret de Villard, Crowfoot, Kirwan and Griffith, the author's recent work at Soba and Ghazali gives it authority, to say nothing of his earlier field-work lower down the river. How many people realize that Nubia, i.e. the Northern Province of the Sudan, was Christian and used Greek letters for nearly a millennium?—from the middle of the 6th century to soon after 1323. The pamphlet is well illustrated by plans of churches, drawings of wall-paintings and of inscriptions, but we miss a plan of the most famous church, that of Old Dongola, which to our shame still awaits description. O.G.S.C.

A NEW SURVEY OF ENGLAND: DEVON. By W. G. HOSKINS. pp. xx+600, with 59 plates and 18 maps. Collins, London, 1954. £2 2s.

This is the second volume of the new survey of England to appear. The survey, to quote the General Editor, Professor Simmons, 'has two main objects. It is intended in the first place to describe the local history of England, relating it to the history of England as a whole; and secondly to give some account of the country as it is today, linking past and present together'. The book consists of two parts. Sixteen chapters, extending to three fifths of the whole, relate to the history, principally the economic and social history, of the county. The remainder is formed of a series of short descriptions of individual places, arranged alphabetically. Exeter is discussed only in so far

as an outline of its development and functions is indispensable to an understanding of the county as a whole. This city and a number of others have been allotted separate volumes in the survey.

The general chapters more than fulfil the hopes raised by Dr Hoskins' contributions to the Devonshire Studies, published in 1952 in collaboration with Mr Finberg. They discuss, concisely and with learning, such subjects as the development of industry and trade, the extension of cultivation in the Middle Ages, the growth of population and the gradual improvement of communications. The survey starts with the prehistoric and Roman ages and is carried down to the building of the railways and the political history of the 19th and 20th centuries. The footnotes are massed at the end of the book and the range of authorities cited shows that the author has missed no important source. The result is a story of enthralling interest both to the specialist and to the educated layman seeking to learn something more about his county.

The descriptions in the second part are less satisfying. The 200 pages allotted are insufficient to give an adequate account of each village and the space is unevenly allocated. 'I must confess that I have occasionally spread myself over a place that has always possessed for me a special fascination, where I have felt the genius loci in its fullest power, as for instance in the little riverside town of Topsham'. Such places and many others are well served, but some of the less fortunate have to be content with a rather barren catalogue. Even so the wealth of detail in these accounts must make the reader wonder at the industry that has achieved their compilation. Errors there must inevitably be, but the very few we have noted are essentially points of minor detail.

The plates grouped in the centre of the volume are admirably chosen. Of the 59 half tones, the greater number are excellent modern photographs. The series starts with several reproductions of early drawings and paintings, a source, the value of which is rightly stressed by the author. The maps also include several redrawn from early publications. A review can do no more than indicate the treasure which this volume contains. We would urge all who are interested in Devon to read the first part; the second will serve as a guide when touring the county and will lead to unexpected pleasures.

C. A. RALEGH RADFORD.

MYTH OR LEGEND? *By Twelve Authors. pp. 126, with 19 plates. London: G. Bell and Sons Ltd., 1955. 10s. 6d.*

These twelve talks given over the B.B.C. in 1953 and 1954 discuss whether certain well known stories are myth, i.e. invented, or legend, having a basis of historical truth. The stories are Lyonesse (Dr G. E. Daniel), Troy (Professor D. L. Page), Glastonbury and the Holy Grail (Professor R. F. Treharne), the Flood (Sir Leonard Woolley), Theseus and the Minotaur (Dr C. T. Seltman), Tara (Professor S. P. Ó Riordáin), Tristan and Iseult (Mr J. M. White), St. George (Mr E. R. Leach), the Isles of the Blessed (Mr T. C. Lethbridge), the Druids and Stonehenge (Professor S. Piggott), Atlantis (Mr J. S. P. Bradford) and Nemi and the Golden Bough (Professor H. J. Rose). The difference in the subjects chosen necessarily imposes a wide difference of approach, and not all the authors use the terms myth and legend with the technical exactitude indicated in the above definitions, which are taken from Dr Daniel's preface. The popular, but not unscholarly, revaluation of these well known tales illustrate the new approach to proto-history through a combination of the evidence from written records with that provided by archaeology. There are useful notes on books recommended for wider reading at the end of each chapter.

C. A. RALEGH RADFORD.

THE WORLD MAP. By RICHARD OF HALDINGHAM in *Hereford Cathedral circa* A.D. 1285, with *Memoir* by G. R. CRONE. (*Reproductions of early Maps III*). London Royal Geographical Society, Kensington Gore, London, 1954. 9 sheets and a supplementary sheet; memoir of 30 pp. Price £3 10s. post free.*

The *Mappa Mundi* (world-map) of Richard of Haldingham, preserved in Hereford Cathedral, where it was first recorded in 1682, is one of the most famous and imposing monuments of medieval cartography. Drawn on a single sheet of vellum, it measures 64 by 54 inches (1.62 by 1.37 m.). In 1830 the Geographical Society of London published a full-size facsimile. This was used by Jomard and Santarem and for the monograph of Bevan and Phillott, who in 1873 transcribed the names and notes. But the best known account of the map is that of Konrad Miller, who, in the 4th part of his *Mappae Mundi* (1896), included a coloured illustration. All these publications have a number of errors in the transcription of the names and notes. Today, for the first time, we are presented with an exact photographic reproduction 9/10ths of the original size. This permits a thorough examination of the map and an appreciation of its value.

The *mappa mundi* is circular with Jerusalem at the centre and the east at the top. The earth is shown as a disc surrounded on all sides by the ocean and split up by islands. Around the outer circle are the titles of the four cardinal points; Oriens, Meridies, Occidens, Septentrio. Eight intermediate points are also indicated by circles each with an animal's head in the centre. These form a rose of 12 petals and divide the circle into 12 sectors, following a system attributed by the Greeks to Timosthenes. A short Latin sentence describes the characteristics of each sector.

A note at the bottom serves as a title to the map: *Descriptio Orosii de ornesta mundi sicut interius ostenditur*. *Ornesta* or *ormesta* is a word of uncertain origin, occasionally used in the Middle Ages to describe a *Mappa Mundi*. The allusion to Orosius, whose work *Historiae adversus paganos* is often called *Ornesta Mundi*, shows that the map, or rather its prototype, was intended to illustrate the history of Orosius.

Around the map is a note stating that the measurement of the world was set on foot by Julius Caesar, who entrusted the work to one Nikodoxus for the west, to Theodoxus for the north and east and to Policlitus for the south. Ethicus in his *Cosmographia*, which dates from c. A.D. 500, records a similar tradition, adding that the great work was started in 44 B.C. and took 32 years; he gives the same names, though with slight differences, to the surveyors. But there is no allusion to this important project in contemporary literature. It is interesting to note that the Hereford *mappa mundi* includes the drawing of an Emperor in the act of despatching three persons.

The *mappa mundi* is executed with a wealth of colour. Coasts, rivers, mountain ranges, islands, etc. are shown in a purely conventional manner, with no reference to their real shape, size or direction, so that the greater number would be unidentifiable without the names and notes. Towns and villages are indicated by a great variety of symbols, from elaborate designs for the greater cities such as Rome, Paris, Antioch and Constantinople, down to the more modest sign of a simple tower. The simpler symbols are in a classical style and recall those used by Roman surveyors. The names of provinces, generally in red, go back to the administrative reorganization of the Empire carried out under Diocletian.

The map is filled with drawings of animals, monsters, etc., and with notes. Palestine is enlarged so as to appear like a peninsula. The well of Syene, noted for its use by

* This review has been translated from the Italian by Mr C. A. Ralegh Radford, F.S.A., to whom we express our thanks.—Ed.

Eratosthenes for his celebrated measurement of the earth's dimensions, is shown so large that its position is entirely misleading.

While the names and notes are all in Latin, there is in one corner of the map a note in Norman French. This gives the name of the author, Richard of Haldingham and Lafford, *ki la fet e compassé*, a sentence which appears to mean: who has drawn and measured it. The author can be identified with a certain Richard de Bello, who is recorded in connection with the parish of Lafford (of which Haldingham is a part) and with Hereford Cathedral, between the years 1273 and 1313. The attribution is therefore certain. In the opinion of palaeographers the writing and drawing of the map are the work of a single hand; the script is an English gothic of the end of the 13th century and there are no later additions.

Crone does not reproduce all the notes, which have already been transcribed by Bevan and Phillott, but a supplementary sheet conveniently provides the identification of all the places, which are entered on a modern map. On the same sheet are shown the layout of two older maps, a *mappa mundi* of Isidore (c. A.D. 775) and one of Henry of Mainz (1110).

The analysis of the contents penetratingly carried out by Crone, leads to important conclusions. The 13th century saw a renaissance in medieval cartography, but the Hereford *mappa mundi* shows no trace of this. Apart from a very small number of points, the map might be attributed to the 7th or 8th century. The prototype is even older; it may have been a *mappa mundi* of the age of Diocletian, later modified on the basis of one or more itineraries. The choice of places suggests that originally the principal purpose of the map was to show the roads. The analogies with the work of Orosius are clear. A revision, or rather a modernization, was carried out in the 12th century, but only in respect of the British Isles and a part of France. Some information taken from Adam of Bremen was added; the legend of St. Brendan was inserted; the earthly Paradise was drawn in the Far East.

If it be admitted that the archetype goes back to the 4th century, there arises the problem of its relation to the well-known *Forma Orbis* of Agrippa, which exercised so great an influence on later cartography. Crone reaches no definite conclusion on this point, largely because so little certain is known about the *Forma*. It is even doubtful whether it was rectangular, like the *Tabula Peutingeriana*, oval, as some modern scholars have argued, or circular as maintained by Uhden, a first-class authority on ancient and medieval cartography. But arguments in favour of at least an indirect relationship with the map of Agrippa emerge from the cautious comments of Crone.

Finally the conclusion emerges that the Hereford *mappa mundi* is a document of intense interest and that possibly its careful examination may lead to a general re-evaluation of the medieval cartographic tradition and its relation to that of the classical world.

ROBERTO ALMAGIA.

THE SETTLEMENTS OF THE CELTIC SAINTS IN WALES. By E. G. BOWEN. pp. x+176, with 53 figs. Cardiff: University of Wales Press, 1954. 10s 6d.

Professor Bowen holds the Gregynog Chair of Geography and Anthropology at Aberystwyth and this book represents an attempt by a historical geographer to elucidate one of the problems of early Wales. The first part studies the connections and spheres of influence disclosed by recorded dedications to the Celtic saints. The second is concerned with the siting of the early churches and their relation to the general pattern of settlement.

REVIEWS

One important matter calls for comment at the outset. Scholars have long recognized that the churches of Wales—and indeed of all the Celtic lands—differ from those of England and Western Europe in the high proportion of dedications to local saints, generally persons unknown outside the Celtic world. This has normally been considered a primitive feature, representing a peculiarity of the Celtic Church. Professor Bowen follows this view but he nowhere fully examines the evidence. In the present reviewer's opinion these dedications represent a primitive feature that should be compared with the primitive Christian custom, attested in the Roman *tituli* and elsewhere, of naming a church after the secular founder. By extension a church might be named after the founder of a monastery, if it was in turn established by a member of the community. An explanation on these lines would bring the Celtic Church into line with conservative Continental practice at the time of the conversion in the 5th century. Only later would these designations of the churches become dedications in the normal medieval sense. If this explanation be accepted it would involve some modification of the details of Professor Bowen's thesis, but it would leave intact the main arguments and above all the most important demonstration of the spheres of the early Christian Celtic world which are so well brought out. The connection of North Wales with Strathclyde illustrated by the cult of St. Kentigern and the separation of North and South Wales, to take two instances, are already attested by other evidence and it is valuable to have this confirmation from a further field of study.

The second part of the volume with its detailed analysis of the location of the early churches is an outstanding example of the geographical method and its value for history. We feel that the eremitical character of the churches other than the greater monasteries has possibly been overstressed. Many of the parish churches of Wales probably arose in clearings used for public worship and above all for public burial; these clearings only later became the site of church buildings. They would not necessarily have been remote from contemporary settlements; in fact we should expect this remoteness to be the exception rather than the rule. The argument on pp. 112-16 is not entirely convincing in that it does not in our view allow sufficient weight for the probability that the farm sites contemporary with the foundation of the churches have in many cases been continuously occupied. This is a minor point concerning the interpretation of the evidence; all students of this period have cause to thank Professor Bowen for the fullness and clarity with which he has presented the material.

C. A. R. RADFORD.

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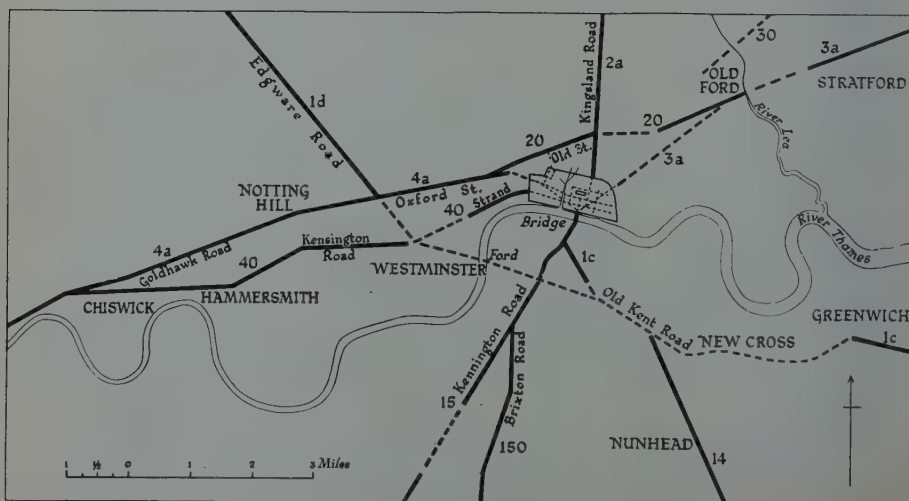
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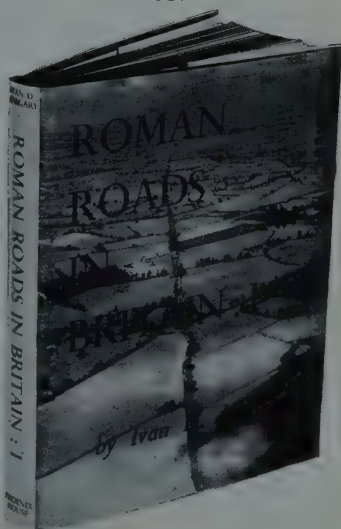
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Edited by

O. G. S. Crawford, C.B.E., Litt.D., F.B.A.

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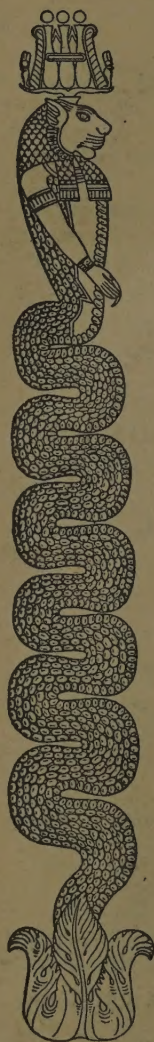
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